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HERMANN EBBINGHAUS

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Hermann Ebbinghaus[1] was born just about nine months (January 24th) before the famous October morning in 1850 when Fechner had his vision of the principle of Weber's law.[2] He was the son of a merchant, Carl Ebbinghaus, in the town of Barmen,[3] Germany. Of his infancy and childhood I have been able to discover nothing except that he was brought up in the Evangelical faith and attended the town *Gymnasium* until he was seventeen. In 1867 he went to the University of Bonn, and somewhat later to Berlin and Halle.[4] Although his initial interests were history and philology, he was gradually drawn into philosophy. The outbreak of the Franco-Prussian War in 1870 interrupted his university work. He joined the Prussian Army in which he remained until the spring of 1871, when he returned to Bonn to continue his philosophical studies. On August 16th, 1873, he obtained his Ph.D. with the dissertation, *Über die Hartmannsche Philosophie des Unbewussten*, after a "glanzend bestandenem Examen."[5]

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[1]This paper is a revision and condensation of a paper presented in Dr. E. G. Boring's Seminary on the History of Experimental Psychology, at Harvard University, April 2, 1928. I wish to acknowledge with thanks the interest and suggestions of Professor Boring.

[2]Cf. E. G. Boring, *A History of Experimental Psychology*, 1929, 270.

[3]Close neighbor of Elberfeld, famous in psychological circles for some of its equine inhabitants.

[4]An attempt to determine the exact time spent in each of these places has thus far been unsuccessful. An odd issue of the Berlin Register for 1867-1868, which I have unearthed, does not contain Ebbinghaus' name.

[5]Karl Marbe, *Frankfurter Zeitung*, Literaturblatt vom 7. Marz, 1909. This estimate is not based on personal observation as Marbe was only four years old at the time.

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An intellectual lifework appealed strongly to Ebbinghaus at the time. He therefore planned to return to Berlin to prepare himself further for an academic position. These plans did not materialize, however, for we find him in England and France as a student and teacher from 1875 to 1878 and acting as French tutor to a Prince Waldemar during 1879.

In 1880 he habilitated himself at Berlin. While there, he founded the laboratory[6] and in 1885 published *Über das Gedächtnis*. He was made *ausserordentlicher Profesor* in the same year, probably in recognition of the latter accomplishment. In association with Arthur König he founded the *Zeitschrift für Psychologie und Physiologie des Sinnesorganes* in 1890. Breslau called him in 1894 to become a full professor in the chair left vacant by Theodor Lipps' going to Munich to replace Stumpf who was bound for Berlin. At Breslau Ebbinghaus again founded a laboratory. In 1905 he had a call—this time to Halle—to succeed Alois Riehls who was going to Berlin. He remained there as Professor of Philosophy until his sudden death from pneumonia on February 26, 1909, just a month after he had entered his sixtieth year.

Ebbinghaus found his own way to psychology and beat his own path. None of his instructors determined, in any marked way, the direction of his thinking, despite the fact that among them there were such eminent individuals as J. E. Erdmann, F. A. Trendelenburg, and J. B. Meyer. On the other hand, the com-

[6]Certainly before 1891; "in the late eighties," says J. McK. Cattell, Early psychological laboratories, *Science*, 67, 1928, 546. Wundt founded the Leipzig laboratory in 1879. The formal date for G. E. Müller's laboratory at Göttingen is 1888, and Müller has said that it was the first laboratory in Prussia. Münsterberg's experimental researches at Freiburg, which began in 1887-88, had to be done in rooms in Münsterberg's own house; cf. Margaret Münsterberg, *Hugo Münsterberg, His Life and Work*, 1922, 27. There was nothing at Halle, where Stumpf used the cathedral organ for his work on tonal fusion; Stumpf, however, dates the rudimentary laboratory at Munich from his advent there in 1889: cf. C. Stumpf, in R. Schmidt, *Die Philosophie der Gegenwart in Selbstdarstellungen*, 5, 1924, 217. Lipps had an unofficial laboratory at Bonn in space loaned by the physicists, and it probably dates from about 1887; cf. W. O. Krohn, this JOURNAL, 4, 1892, 592 f. This same article describes Ebbinghaus' laboratory at Berlin, pp. 589 f. There were no others in Germany. It is plain that Ebbinghaus was experimenting at Berlin before there were formal or informal laboratories elsewhere in Germany except at Leipzig. If Göttingen really antedated Berlin, then Ebbinghaus' laboratory was the third in Germany, except for Münsterberg's and Lipps' unofficial laboratories at Freiburg and Bonn. Stumpf's account of the founding of the Berlin laboratory with his advent in 1894 is therefore misleading; cf. Max Levy, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, III, Halle, 1910, 202-207. (I am indebted to Professor Boring for these data.)

bination of philosophical and scientific points of view, which he found in Fechner, a copy of whose *Elemente* he picked up in a Parisian second-hand bookstall, influenced him greatly. This debt he acknowledged in his dedication of the *Grundzüge* to the memory of Fechner. Gracefully put in verse, it ends, "ich hab'es nur von Euch."

One *Leitmotif* runs through his work from his very first publication: psychology is *Naturwissenschaft.* The first thesis in his dissertation (1873) sets forth the proposition: "Psychology (in the broadest sense) belongs no more to philosophy than does natural science."[7] In *Über das Gedächtnis*, in a discussion of the possibility of an experimental approach to the memory problem, he says, "at the very worst we should prefer to see resignation arise from the failure of earnest investigation rather than from the persistent, helpless astonishment in the face of their difficulties."[8] Thus he spent his energies in an attempt to establish psychology on a quantitative and experimental basis.[9] His estimate of the value of experiment for psychology is well brought out in the short historical sketch which introduces his *Abriss der Psychologie.* "When Weber in 1828 had the seemingly petty curiosity to want to know at what distances apart two touches on the skin could be just perceived as two, and, later, with what accuracy he could distinguish between two weights laid on the hand, ... his curiosity resulted in more real progress in psychology than all the combined distinctions, definitions, and classifications of the time from Aristotle to Hobbes (inclusive)."[10] The desire to bring into psychology clear and exact methods resulted in unusual carefulness in experimental technique and considerable interest in apparatus.[11]

Ebbinghaus was an unusually good lecturer. His ever-youthful personality, his natural humor, and the unusual clarity and ease

[7]Ebbinghaus was merely making articulate the attitude finding expression in others at the same time (*e.g.* Wundt, in the first edition of his *Physiologische Psychologie*). The seeds sown by E. H. Weber, Fechner, Johannes Müller, *et al.*, were sprouting fast.

[8]English trans., 6.

[9]In this connection it is interesting to note a criticism by him (in *Zsch. f. Psychol.* 3, 1892, 201) of an article by W. T. Harris on "Fruitful lines of investigation in psychology" published in the *Educational Review*, 1, 1891, 8-14. In this article Harris warned educators against the materialistic psychology, pointing out that only in a study of the soul is psychology of value to education. Ebbinghaus ends his short abstract with the caution, "Let us hope that people will be warned and stay away from such a wicked science."

[10]English trans., 17.

[11]Cf., *e.g.* Bibliography, No. 19, *infra.*

of his presentation assured him of an overflowing audience and of considerable influence on his auditors. At meetings of the *Psychologische Gesellschaft* these characteristics and the wide range of fields at his command made him a leading participant in discussions. Another outstanding trait, which was particularly valuable in his rôle of editor of a journal, was his Jamesian tolerance.[12] The special importance of permitting the expression of the most diverse opinions in a young science was taken for granted by him. He seemed even to encourage sincere opposition, especially in younger men. In personal contacts with his students, he invariably manifested the deepest interest and affection, especially in the discussion of their experimental problems.

In view of the unusual attractiveness of this man it is surprising that the number of his disciples is so small. Jaensch accounts for this by the fact that Ebbinghaus had no desire to develop disciples.[13] He never urged another to undertake an investigation; in fact, if one wanted to work with him one had to obtrude one's self upon him with firmness.[14] Some of his better known pupils are A. Wreschner, L. W. Stern, and O. Lipmann.

Besides his courses in various branches of psychology, Ebbinghaus continued throughout his pedagogical life to give lectures in philosophy and in aesthetics. These were, however, only of minor interest to him. Psychology was by far his major interest. He either founded or developed the laboratories at all of the universities at which he taught. He founded the laboratory at Berlin, but evidently he did not get very much support from the University, for Krohn reported in 1891 that Ebbinghaus had two rooms with little apparatus.[15] When he went to Breslau in 1894 he founded a laboratory there.[16] At Halle, to which he was called in 1905, he enlarged and developed the very modest laboratory which he found there. Stumpf had been there from 1884-1889, but he had evidently done nothing in the way of founding a laboratory.[17]

[12]Boring has called Ebbinghaus the 'James of Germany' (with the reservation: 'if there can be another James anywhere!') The similarity of these two men in a number of respects is indeed striking and worthy of special study. Cf. E. G. Boring, *op. cit.*, 383-385.

[13]E. R. Jaensch, Hermann Ebbinghaus, *Zsch. f. Psychol.*, 51, 1909, i-vii.

[14]Compare in this respect, Cattell's accounts of Wundt's method, *Psychol. Rev.*, 28, 1921, 156; and *Science*, 67, 1928, 545.

[15]Krohn, *op. cit.*, 589-590. Cf. also Stumpf's account in Max Levy, *loc. cit.*

[16]Note in this JOURNAL, 7, 1895-1896, 152.

[17]Krohn found none there in 1891; *op. cit.*, 592.

Ebbinghaus did psychology an important service in founding and editing the *Zeitschrift*. The fifty volumes published up to his death present a practically complete portrait of the psychology of the two decades from 1890 to 1910.[18] Since Ebbinghaus was entirely free of schoolishness the researches and opinions of every group found its way to his columns. A glance through the index of names and studies is enough to convince one that the *Zeitschrift* was the most important psychological organ in Germany, and for that reason, in the world.[19] Helmholtz, Stumpf, G. E. Müller, Schumann, Th. Lipps, Von Kries, are only a few of the names which strike one in a cursory glance at the contents of a few volumes.

Since this paper is concerned solely in tracing the intellectual and personal history of Ebbinghaus and not in expounding his work and theories, only his more fundamental attitudes and the more general nature of his contributions will be treated in the following discussion.

Compared to Wundt, Ebbinghaus was an amateur in the matter of publication.[20] His published work is comparatively small. His own point of view with regard to print is expressed in a passage quoted by Woodworth,[21] "the individual has to make innumerable studies for his own sake. He tests and rejects, tests once more and once more rejects. For certainly not every happy thought, bolstered up perhaps by a few rough and ready experiments, should be brought before the public. But sometimes the individual reaches a point where he is permanently clear and satisfied with his interpretation. Then the matter belongs to the scientific public for their further judgment." How serious Ebbinghaus was in this attitude is shown in the matter of his memory experiments. Although they were completed in 1880, he did not report the results until 1885 after having repeated them in their entirety in 1883.[22]

Von Hartmann's *Philosophie des Unbewussten* had come out in 1869 and had achieved numerous editions within a few years.

[18]With the possible exception of the Wundtian School products, which were being published almost entirely in *Philosophische Studien*.

[19]Cf. Karl Bühler, *Die Krise der Psychologie*, 1927, 1-2.

[20]May this be partially accounted for by the fact that Ebbinghaus probably had no typewriter? Cf. Cattell, *Science* (*loc. cit.*), and Dürr's Introduction to Ebbinghaus' *Grundzüge* in which he refers to the "manuscript" he found.

[21]R. S. Woodworth, Hermann Ebbinghaus, *J. Philos.*, 6, 1909, 255. I have been unable to trace the source.

[22]English trans., 33.

It was therefore entirely natural that it should be a popular subject for dissertations among doctoral candidates of the period. Ebbinghaus' treatment of it was very critical, as was to be expected from one who, in the same dissertation, defended theses setting forth the essential similarity of psychology and the natural sciences and the abstract and verbal nature of the then-existing psychology. "Wherever the structure is touched, it falls apart,"[23] and "What is true is alas not new; the new not true,"[24] are some of the conclusions at which he arrived.

Über das Gedächtnis: Untersuchungen zur experimentellen Psychologie was published in 1885. It is to be deplored that Ebbinghaus left no record[25] of the period preceding the memory work. In the introduction to the section on nonsense syllables he made the bare statement, "In order to test practically, although only for a limited field, a way of penetrating more deeply into the memory processes . . . . I have hit upon the following method,"[26] and he went on to discuss the nature and mechanics of nonsense syllables.

Let us, with the few facts and the considerable conjecture at our command, attempt to reconstruct in at least a sketchy way the background for this 'hit.' Accepting as authentic the facts given in the short biographical accounts available, we know that in 1873 he received his Ph.D. We have no record of what he did between 1873 and 1875, except that he had planned to go to Berlin for further work—which he may have done. From 1875 to 1878 he taught, travelled, and studied in England and in France. During part of 1879 he tutored a young prince in French.

According to a statement in *Über das Gedächtnis* the work was first done in 1879 and 1880, with a "long time" of preliminary experimentation.[27] We know, therefore, that Ebbinghaus must have 'hit' on his method *before* 1879. Now to get at the other limiting date! Jaensch tells of Ebbinghaus having picked up a copy of Fechner's *Elemente* in a Parisian second-hand bookstall, which, Jaensch says, inspired the memory work.[28] This seems likely to have been some time during the years 1875-1878; and it

[23] *Über die Hartmannsche Philosophie des Unbewussten*, 59.
[24] *Ibid.*, 67.
[25] At least no published record that I have been able to discover.
[26] English trans., 22.
[27] English trans., 33.
[28] Jaensch, *op. cit.*, v.

also seems reasonable to suppose that this was Ebbinghaus' first acquaintance with the *Elemente* and perhaps Fechner, for it does not seem likely that Jaensch would have mentioned this find if Ebbinghaus had not attached considerable importance to it. Ebbinghaus' telling of the fact was evidently more than the bibliophile's boast of a bargain picked up in a book, copies of which were probably fairly common in second-hand bookshops.

So it is likely that between 1875 and 1879, nonsense syllables, for use in memory work, first came into being.[29] That is about as near as we can safely come with the knowledge at our command. As to the exact process of genesis, we can only guess. Was it an invention in the commonly accepted sense of the term, that is to say, deliberate? Or was it largely a discovery? What part did the gurgle of an infant, a transient regression to infancy, the reading of Jabberwocky,[30] the expletives of the Paris coachman or the London cabby, play? Probably more than Ladd and Woodworth allow for in the hypothetical account they give in their discussion of the reasoning process.[31]

What of the significance of *Über das Gedächtnis*? Previously, exact work had been limited to problems of predominantly physiological affinities. It is not that there was no interest in more strictly psychological fields,[32] but now a fundamental central function had been *experimentally* investigated! It *must* have meant a good deal to the young science, although comparatively little of

[29]Ebbinghaus was probably *eingestellt* at that particular time to the problem of memory which he got from the British Associationists. Cf. Boring, *op. cit.*, 380.

[30]"Through the Looking Glass" had been published in 1872, and had achieved considerable popularity. Perhaps Ebbinghaus acquired proficiency in English by reading Lewis Carroll!

[31]G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 604 f.

[32]In this regard it is of interest to compare some statements made by Clemens Baeumker in his *Selbstdarstellung: Philosophie der Gegenwart in Selbstdarstellungen*, II, 38-39: "From the study, especially of Wundt's *Physiologische Psychologie*, the idea grew on me to apply the experimental method to higher psychic processes, at first to memory. For a long time I undertook experiments for this purpose without finding, to be sure, the best means for it, methodically built syllables. Instead of these I used digits. . . . . But when in 1885 the pioneer work of Ebbinghaus on memory appeared, which by means of a methodical technique brought superlative results, in discouragement I let my own plans fall."

the contemporary effect is to be discovered in print.[33] James[34] was impressed with the "heroic" nature of the experiment, as was Tanzi.[35] Later opinion is probably fairly expressed by Titchener:[36] "It is not too much to say that the recourse to nonsense syllables, as a means to the study of association, marks the most considerable advance, in this chapter of psychology, since the time of Aristotle." Nevertheless, Titchener also thought that,[37] "the introduction of nonsense syllables . . . . has nevertheless done psychology, a certain disservice. It has tended to place the emphasis rather upon organism than upon mind."[38]

Between 1887 and 1890 Ebbinghaus published three papers[39] discussing the revision of the Weber-Fechner law and offering a physiological interpretation[40] which would account for the failure of the law to hold at the extremes.

In 1892 he set forth his *Theorie des Farbensehens*,[41] which on the whole was not favorably received. In his *Grundzüge* he gave very little space to it, evidently not taking it very seriously.[42]

Although Ebbinghaus was loath to enter into controversy, it was natural for him to take up the cudgels for psychology as he understood it, when Dilthey's *Ideen über eine beschreibende und*

[33]This may in large part be accounted for, perhaps, by the dearth of psychological journals. *Mind* and *Philosophische Studien* were the only more strictly psychological journals in existence. The former contains a review by J. Jacobs (*Mind*, 10, 1885, 454-459), which is very favorable. The latter (founded in 1883) carried no reviews. Besides this I have, however, been able to discover the following reviews:

Joh. Volkelt, *Zsch. f. Philos. u. philos. Krit.*, 93, 1888, 126-137 (very favorable).

A. Höfler, *Vjsch. f. wiss. Philos.*, 2, 1887, 340-351 (very favorable).

Ed. Schmidt, *Rev. Philos.*, 19, 1885, 687-693 (favorable).

Otto Flügel, *Zsch. f. exacte Philos.*, 14, 1886, 166-172 (favorable).

A. Elsas, *Philos. Monatshefte*, 23, 1887, 84-88. Elsas was sorry not to be able to criticize more favorably this very elaborate work undertaken in the true service of science, but felt that "not the author, but the school from which he comes, has to answer for the book." Cf. in this relation, Elsas, *Über die Psychophysik*, 1886.

[34]*Principles*, I, 1890, 676.

[35]*Rivista di filosofia scientifica*, 4, 1885, 598-600. "Una serie di ricerche, veramente digne d'un Certosino [A series of researches, truly worthy of a Carthusian monk]." The review on the whole is very favorable.

[36]*Text-Book of Psychology*, 1910, 380-381.

[37]*Ibid.*, 414.

[38]A statement, which would at the present time make for more general concurrence in the former judgment!

[39]See Bibliography, Nos. 3, 4, and 6, *infra*.

[40]James called this the most 'real' hypothesis of Weber's law on the neural side (*Principles*, I, 548).

[41]Cf. Bibliography, Nos. 8 and 9, *infra*.

[42]Vol. I, 1902, 260 f.

zergliedernde Psychologie appeared.[43] The keystone of Ebbinghaus' faith was being attacked! In an article in the *Zeitschrift*, *Über erklärende und beschreibende Psychologie*,[44] he justified the use of hypothesis and causal explanation in psychology.

Dilthey held that the new psychology could never be more than descriptive and that attempts to make it explanatory and constructive were wrong in principle and lead to nothing but confusion of opinion and fact. "We explain nature, but we understand psychic life;" and any psychology which was modelled after atomistic physics—as was a psychology such as Ebbinghaus'—could never "understand," for in the final analysis the process of "understanding" had to be experienced (*erlebt*) and could not be inferred logically (*erschlossen*).

Ebbinghaus in his reply claimed that, in so far as Dilthey was attacking explanatory psychology, he was attacking the old associationists, as to whose failure he agreed. Their difficulty arose, he felt, because of a use of the fields of chemistry and physics as fields of analogy rather than that of biology. Dilthey was not actually discussing the modern psychology: he talked of "the" explanatory psychology but was mostly using Herbart as an example—and Herbart was dead, even in Germany. Outside of Germany he never had had very much influence[45] because of his "metaphysical sophistries, unfounded fictions, and mythology." With Dilthey's point that explanatory psychology works on the principle that cause equals effect in the physical sense, Ebbinghaus disagreed. All that it can at the present say, and does say, is, "the contiguity of two sensations is considered as causal relationship because later a representation of one sensation results in a *Vorstellung* of the other." The controversy is not over as yet. As Klüver reports,[46] it is one of the outstanding features of the contemporary German psychological map.

In 1895, the school authorities of Breslau were interested in the advisability of holding five-hour school sessions. Ebbinghaus was

[43] W. Dilthey, *Sitzber. d. Berl. Akad. d. Wiss.*, 1894, 99. My acquaintance with it is second-hand through Eisler's *Lexicon* and Gardner Murphy's *Historical Introduction to Modern Psychology*.

[44] Bibliography No. 10, *infra*. As H. Klüver points out (Gardner Murphy, *op. cit.*, 445) many of the issues of the present discussion of "*verstehende*" psychology were first formulated in the Dilthey-Ebbinghaus controversy.

[45] Ebbinghaus is evidently referring here to his influence on psychologists rather than on educators.

[46] *Op. cit.*, 455.

one of a commission appointed to investigate this problem. His contribution was the *Combinationsmethode.* Although this device was not very successful in measuring intellectual fatigue, it was found to be very valuable as a measure of general intellectual capacity, since it correlated highly with the rank and scholarship of the pupils. Current adaptations of the principle involved are found in two such psychometric standbys as the Healy Picture Completion Test and the Trabue Sentence Completion Test.[47]

When Ebbinghaus died in 1909, the systematic treatise which he had started at the beginning of the last decade of the previous century was only a little more than half completed. The first half of Volume I had come out in 1897. This volume was first published as a whole in 1902, and a second edition of it followed in 1905. In 1908, the first section of Volume II (96 pages) appeared. On Ebbinghaus' death Dürr took over the editing of his works. He found about forty pages of manuscript, which broke off in the midst of a discussion of convergence and accommodation, and nothing more in the way of notes.[48] Dürr completed the second volume on the basis of his own views and the current psychological literature.[49] In 1911, sections 2 and 3, and in 1912, sections 4 to 7 appeared. The second volume had its first complete appearance in 1913, and the first volume achieved a third edition in 1911.

The *Grundzüge* was enthusiastically received,[50] which is not at all surprising, for besides a masterly, concise and clear presentation of the existing psychology there were numerous suggestive criticisms and improvements of current theories. On the whole, its contribution was in its readableness and in its general format rather than in any radical approach to psychology.

Abriss der Psychologie is the title of a two-hundred-page sketch of psychology published by Ebbinghaus in 1908. Originally written for Hinneberg's *Kultur der Gegenwart* as the contribution

[47]In 1909 Woodworth (*loc. cit.*) said of the Ebbinghaus Completion Test: "Probably it has greater claims to be regarded as a test of intelligence than any other single test that has been introduced."

[48]Karl Bühler in personal conversation has said that Ebbinghaus kept few notes.

[49]Cf. Preface to Vol. II.

[50]M. Foucault, in *Rev. Philos.*, 55, 1903, 329-341, in a generally favorable criticism, talks of the section on Sensation as "vraiment magistrale!" McDougall, in *Mind*, N.S. 11, 1902, 578 f., in a review of the second half of Volume I, says it "bids fair to rank as the best general text-book for all classes of students, beginners not excepted."

on Psychology,[51] it was found too long, and was cut in half for inclusion in the volume. It was published as a whole as the *Abriss*. After the first edition on the average of more than one new edition came out every two years until the last one,[52] the eighth, in 1922. Dürr acted as editor of it for a time, and then Karl Bühler. An English translation by Max Meyer appeared in 1908, and French editions in 1910 and 1912. Its value and appeal may be judged by these facts. An admirable short sketch of the history of psychology introduces the volume, which begins with the oft-quoted, "Psychology has a long past but only a short history."

What has Ebbinghaus done for psychology? Where does he belong? He has contributed much and had great influence, mostly in an indirect way. *Über das Gedächtnis* is undoubtedly his outstanding contribution. His memory work was not only the starting-point for practically all the work which has followed in this field, but probably also for the work in the acquisition of skill. His *Grundzüge* comes next, not for its new system (which is very much like that of other psychologists who come to mind) but for its clear and concise treatment of the literature and its experimental emphasis.[53] His *Combinationsmethode* has meant a good deal to the field of mental testing. His editing of the *Zeitschrift* did much to advance psychology in a very productive period. His emphasis on experiment and his faith in the laboratory approach resulted personally in the establishment of at least two laboratories and the development of a third. His qualities as a lecturer and writer did a great deal to spread a knowledge of orthodox psychology.

Despite an early training in philosophy, he was one of the leaders in the movement to emancipate psychology from philosophy. He belongs fundamentally in the tradition which leads from pre-psychological science, through physiology, Helmholtz, and Fechner, to Wundt and 'content psychology.' Dunlap would give him, together with Aristotle and Binet, the credit for making

[51] Among other contributors were Wundt, Eucken, Lipps, and Riehl.

[52] At least that is the last of which I can find a record.

[53] Cf. Titchener in his Clark address, The past decade in experimental psychology, this JOURNAL, 21, 1910, 405: "I have sometimes thought that with allowances made for changed conditions, it [Ebbinghaus' *Grundzüge*] might prove as important for Experimental Psychology even as Wundt's *Physiologische Psychologie* or Brentano's *Psychologie vom empirischen Standpunkte*."

psychology "behavioristic,"[54] but that is probably going too far. His psychology, however, does have a functional emphasis which may be seen in such things as his constant reference to the biological affinity of psychology, his nativism in the matter of general attributes of sensation, and the contribution he has made towards the problem of individual differences.

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THE DEPENDENCE OF TONAL ATTRIBUTES UPON PHASE

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The experiments here reported bear upon experiential changes which accompany modifications in the phase of a sound-wave delivered simultaneously to the two ears.[1] The main problem in the research was to ascertain whether the several attributes of a tone are affected when sound-energy is delivered in unlike phase at the two ears. The obvious effect of phase-difference in binaural hearing is to divert the apparent location of the sound-source right or left from the median plane of the head. We sought to discover whether a change in attributive character also occurred with such a change in spatial location.

Observers were first trained to report in terms of the pitch, volume, brightness and intensity of tones of various frequencies. The observers were then presented binaurally with successive pairs of tonal stimuli which differed only in their phase-relations. They were under instructions to compare the tones with respect to one or another attribute.

The Experimental Study of Auditory Localization

The history of the study of auditory localization may roughly be divided into three overlapping stages; (a) the measure of accuracy of localization (perimetry) together with an analysis of place-errors, (b) the more analytical studies which grew out of theoretical explanations and which attempted to isolate the factors lying at the basis of localization, and (c) the assimilation of the experimental facts regarding localization to current theories of hearing.

(a) In the experiments of the first class, a single or dual sound-source was moved about, often in the open air. With a single source, the direction of the sounding object could be directly estimated and the error of the estimation measured. With a dual sound-source the actual binaural ratio of intensities could be better controlled; but in these earlier experiments the intensive differ-

*Accepted for publication December 1, 1929.
[1]The experiments were carried out at the University of Illinois.

ences were still complicated by other factors introduced by actually changing the position of the sound sources.[2] Localization was generally attributed to binaural intensive differences.

(b) With the clear-cut formulation of the 'intensity' theory[3] there came at intervals the discovery of at least three other sets of conditions aiding or determining auditory localization; differences in the complexity of the sound, differences in the phase-relations of pure tones at the two ears, and slight differences in the time of arrival of a sound at the two ears. Mach is usually credited with first holding that localization may be due primarily to clang-color. Later writers seem to have overlooked his reasons. He pointed out that with low tones the head will throw no appreciable sound-shadow, and hence localization cannot be due to intensive differences.[4] Considerable evidence now points to the importance of the complexity of the sound for accurate localization. Noises and clangs are much more accurately localized than are semi-pure or pure tones.[5] With monaural stimulation, where intensive, phasic, and temporal ratios at the two ears are absent, there is fair accuracy in the localization of noises, but pure tones are localized with practically chance accuracy.[6] This evidence has nevertheless strongly supplemented the 'intensity' theory; while the phasic and time-

[2]Typical studies are: Lord Rayleigh, Acoustical observations, *Phil. Mag.*, (5) 3, 1877, 456-464, and Acoustical notes (viii), *Phil. Mag.*, (6) 16, 1908, 235-246; A. G. Bell, Experiments relating to binaural audition, *Amer. J. Otol.*, 2, 1880, 169-180; H. Münsterberg & A. H. Pierce, The localization of sound, *Psychol. Rev.*, 1, 1894, 461-476; M. Matsumoto, Researches in acoustic space, *Stud. Yale Psychol. Lab.*, 5, 1897, 1-75; C. E. Seashore, Localization of sound in the median plane, *Univ. of Iowa Stud. in Psychol.*, 2, 1899, 46-54; A. H. Pierce, *Studies in Auditory and Visual Space Perception*, Longmans Green, 1901; J. R. Angell, A preliminary study of the significance of partial tones in the localization of sound, *Psychol. Rev.*, 10, 1903, 1-14; E. A. McGamble, The perception of sound direction as a conscious process, *Psychol. Rev.*, 9, 1902, 357-373, and Intensity as a criterion in estimating the distance of sounds, *Psychol. Rev.*, 16, 1909, 416-426; D. Starch, Perimetry of the localization of sound (i), *Psychol. Monog.*, 6, (No. 28), 1905, 1-45, and (ii), *Psychol. Monog.*, 9, (No. 38), 1908, 1-55; D. Starch & A. L. Crawford, Perception of the distance of sound, *Psychol. Rev.*, 16, 1909, 427-430; K. Dunlap, The localization of sounds, *Psychol. Monog.*, 10, (No. 40), 1909, 1-16.

[3]One of the earlier references to this theory is found in A. Steinhauser, The theory of binaural audition; a contribution to the theory of sound, *Phil. Mag.*, (5) 7, 1879, 181-197, 261-274. See also E. W. Scripture, On binaural space, *Stud. Yale Psychol. Lab.*, 5, 1897, 76-80.

[4]E. Mach, Bermerkungen über die Function der Ohrmuschel, *Arch. f. Ohrenhk.*, 9, 1875, 72-76.

[5]This has been demonstrated by Lord Rayleigh, Acoustical observations, *Phil. Mag.*, (5) 3, 1877, 456-464; S. P. Thompson, The pseudophone, *Phil. Mag.*, (5) 8, 1879, 385-390, and On the function of the two ears in the perception of space, *Phil. Mag.*, (5) 13, 1882, 406-416; Angell, *op. cit.*; A. M. Hocart & W. McDougall, Some data for a theory of the auditory perception of direction, *Brit. J. Psychol.*, 2, 1908, 386-405; C. S. Myers, The influence of timbre and loudness on the localization of sound, *Proc. Roy. Soc.*, B, 88, 1915, 267-284.

[6]See Rayleigh, *op. cit.*, and Acoustical observations (iv), *Phil. Mag.*, (5) 13, 1882, 340-347; Bell, *op. cit.*; J. R. Angell & W. Fite, The monaural localization of sound, *Psychol. Rev.*, 8, 1901, 225-246, and Further observations on the monaural localization of sound, *ibid.*, 449-458; Hocart and McDougall, *op. cit.*; and O. Klemm, Untersuchungen über die Lokalization von Schallreizen, *Psychol. Stud.*, 8, 1913, 487-505.

difference theories have, in the views of many investigators, not so much supplemented as actually challenged the theory set in terms of physical intensity.

Although there were several earlier studies on binaural beats and attendant changes in the apparent position of the sound,[7] the theory of sound-localization as based upon binaural phase-differences, which integrated these and other facts, is usually attributed to Lord Rayleigh, who pointed out that the head-shadow is insufficient, at least with frequencies less than 256 cycles, to make an appreciable binaural difference in intensity.[8] It was further urged by More and Fry[9] that such animals as mules and rabbits, whose ears are placed above their heads, have no head-shadow and are nevertheless able to localize. The resulting experimental and theoretical work on localization of tones differing binaurally in phase was carried out in large part by physicists, most of whom concluded that the organism directly perceives differences in phase.[10] It was early shown, however, at least by Wilson and Myers[11] and by Ferree and Collins,[12] that phase is not an experiential term, and that, as a matter of fact, we do not experience the differences as such. Several methods of dealing with phase-differences have appeared. One method consists in reaffirming faith in, and

[7]This aspect of Mach's work (*op. cit.*) seems also to have been overlooked. See also S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391, and Phenomena of binaural audition (iii), *Phil. Mag.*, (5) 12, 1881, 351-355; K. L. Schaefer, Ueber die Wahrnehmung und Lokalisation von Schwebungen und Differenztönen, *Zsch. f. Psychol.*, 1, 1890, 81-98, and Zur interaurealen Lokalisation diotischer Wahrnehmungen, *ibid.*, 300-309; P. Rostosky, Ueber binaurale Schwebungen, *Phil. Stud.*, 19, 1902, 557-598. (More and Fry delayed publishing their work.) L. T. More and H. S. Fry, On the appreciation of differences of phase of sound waves, *Phil. Mag.*, (6) 13, 1907, 452-459.

[8]Lord Rayleigh points out (On the perception of sound direction, *Phil. Mag.*, (6) 13, 1907, 214-232) that the maximal binaural difference at 256 cycles would be about 10% of the total intensity; at 128 cycles it would be about 1% of the total intensity. On the other hand it appears (H. Fletcher, *Speech and Hearing*, Van Nostrand, 1929, part 3, p. 147 ff) that a perceptible increase in loudness does not always involve simply a proportional increase in the sound energy; that, for instance, at a loudness level of 10 decibels the energy must be increased about 73% to be noticeably louder.

[9]*Op. cit.*

[10]Lord Rayleigh, Acoustical notes (vii), *Phil. Mag.*, (6) 13, 1907, 316-333; L. T. More, On the localization of the direction of sounds, *Phil. Mag.*, (6) 18, 1909, 308-319; G. W. Stewart, Phase relations in the acoustic shadow of a rigid sphere; phase difference at the ears, *Phys. Rev.*, 4, 1914, 252-258, The function of intensity and phase in the binaural location of pure tones, *Phys. Rev.*, 15, 1920, 425-445, and The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31, (No. 140), 1922, 31-44; R. V. L. Hartley, The function of phase difference in the binaural location of pure tones, *Phys. Rev.*, 13, 1919, 373-385; R. V. L. Hartley and T. C. Fry, The binaural location of pure tones, *Phys. Rev.*, 18, 1921, 431-442. Fletcher holds that "when the phase is different there will be a corresponding time interval between the occurrence of the two maxima produced in the brain. It is undoubtedly [!] the recognition of this time interval that enables us to recognize phase difference," *op. cit.*, p. 191.

[11]H. A. Wilson & C. S. Myers, The influence of binaural phase differences on the localization of sound, *Brit. J. Psychol.*, 2, 1908, 363-385; C. S. Myers & H. A. Wilson, On the perception of the direction of sound, *Proc. Roy. Soc.*, 80, 1908, 260-266.

[12]C. E. Ferree & R. Collins, An experimental demonstration of the binaural ratio as a factor in auditory localization, this JOURNAL, 22, 1911, 250-297.

adherence to, the 'intensity' theory by demonstrating localizations based upon intensive differences, and discounting the rôle of phase, either directly[13] or indirectly by ignoring it.[14] A second way consisted in attempting to reduce phasic to intensive differences. The famous theory of Wilson and Myers, for instance, attempted to translate phase back into intensive differences by way of bone-conduction between the ears.[15] There is now little, if any, evidence in support of this theory, and very considerable against it. Binaural beats are now usually considered dependent upon central factors, and not upon effects in the peripheral organ.[16] No one has so far authenticated binaural difference-tones.[17] In experiments with cases having monaural deafness no evidence of sound-transmission has been reported, except with great intensities.[18] The masking effects of pure tones has been measured when the two tones were presented to separate ears. At one representative pair of frequencies (1200 and 1300 cycles) the intensity of the 1200 tone at one ear must be increased 1,000,000 times (60 threshold units) before the threshold value of the tone in the other ear is affected. In contrast to this dichotic masking, the masking effect upon the higher tone begins practically with the introduction of the tone of 1200 cycles at the same ear.[19] Again, if the Wilson and Myers hypothesis is true, a

[13]Ferree & Collins, *op. cit.*

[14]G. F. Arps & O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Psychol. Stud.*, 8, 1913, 226-270; C. A. Ruckmick, Experiments in sound localization, *Psychol. Monog.*, 30, (No. 136), 1921, 77-83.

[15]Wilson & Myers, *op. cit.* Several investigations had earlier suggested bone-conduction as a possible theory for the localization of tones differing in phase. See *e.g.* S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, and Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391; Rostosky, *op. cit.*; T. J. Bowlker, On the factors serving to determine the direction of sound, *Phil. Mag.*, (6) 15, 1908, 318-332. A careful history of the general problem may be found in H. Banister, The transmission of sound through the head, *Phil. Mag.*, (7) 2, 1926, 144-161, and Phase effect and the localization of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431.

[16]J. Peterson, The nature and probable origin of binaural beats, *Psychol. Rev.*, 23, 1916, 333-351; G. W. Stewart, The theory of binaural beats, *Phys. Rev.*, 9, 1917, 514-528; E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[17]S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276; K. L. Schaefer, Ueber die Wahrnehmung und Lokalisation von Schwebungen und Differenztönen, *Zsch. f. Psychol.*, 1, 1890, 81-98; C. R. Cross & H. M. Goodwin, Some considerations regarding Helmholtz's theory of consonance, *Proc. Amer. Acad. Arts & Sci.*, 27, 1891, 1-12; Peterson, *op. cit.*, and H. Banister, The transmission of sound through the head, *Phil. Mag.*, (7) 2, 1926, 144-161.

[18]L. T. More, *op. cit.*, Peterson, *op. cit.*, Banister, *op. cit.* R. L. Wegel & C. E. Lane (The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear, *Phys. Rev.*, 23, 1924, 266-285) got the tone across when the intensity was very great. They attribute this to bone-conduction.

[19]H. Fletcher reports this in practically identical language in Physical measurements of audition and their bearing on the theory of hearing, *J. Frank. Inst.*, 196, 1923, 289-326, and *Bell Sys. Tech. Jour.*, 2, 1923, 145-180, and *Speech and Hearing*, part 3. See also Wegel and Lane, *op. cit.* The effect is again attributed to bone-conduction. Banister (Phase effect and the localization of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431) points out, however, that they have overlooked several other possible conduction sources, such as the sinuses.

perceptible intensive difference should be observable between phase-differences of 0 and $\lambda/2$; yet such differences are not obtained.[20] Finally, those studies where intensity alone has been varied at the two ears indicate that the intensive difference must be many times greater than naturally occurs, in order to obtain lateral localizations.[21] It was this type of evidence that drove certain physicists to the theory that we perceive phase directly.[22] We may conclude that localizations can be made upon the basis of binaural intensive differences alone and upon binaural phasic differences alone, but that the one is not reducible to the other.

The third way of dealing with phase comes from those recent experiments which show that dichotic clicks slightly differing in the time of arrival at the two ears are perceived as a single sound localized laterally in proportion to the time-difference.[23] This result is used to support the suggestion that phase-differences can be reduced to temporal differences,[24] as opposed to the view that the ratio of the phase-difference at the ears to the angular displacement of the fused sound is proportional to the frequency of the pure tone employed.[25] But this is a facile solution which merely pushes back the problem another step, since the temporal difference is not perceived but instead a single tone or

[20]H. Banister, A further note on the phase effect in the localization of sound, *Brit. J. Psychol.*, 15, 1924, 80-81, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 266-292, and Phase effect and the localization of sound. See also G. W. Stewart, The function of intensity and phase in the binaural location of pure tones, *Phys. Rev.*, 15, 1920, 425-445, and The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31, (No. 140), 1922, 31-44; Hartley & Fry, *op. cit.*

[21]G. W. Stewart & O. Hovda, The intensity factor in binaural localization; an extension of Weber's Law, *Psychol. Rev.*, 25, 1918, 242-251; Stewart, *opp. citt.;* H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, this JOURNAL, 33, 1922, 178-212; H. Banister, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 266-292.

[22]See the references cited in footnote 10 above.

[23]O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Arch. f.d. ges. Psychol.*, 38, 1918, 71-114, and Untersuchungen über die Lokalisation von Schallreizen, *Arch. f. d. ges. Psychol.*, 40, 1920, 117-146; E. M. von Hornbostel & M. Wertheimer, Ueber die Wahrnehmung der Schallrichtung, *Sitzber. Akad. preuss. Wiss.*, 1920, 388-396; J. Whittmann, Beiträge zur Analyse des Hörens bei dichotischer Reizaufnahme, *Arch.f.d. ges. Psychol.*, 51, 1925, 21-122; A. L. Bennett, A measurement of the efficiency of the ears as a means of detecting short time intervals, *J. Opt. Soc. Amer.*, 14, 1927, 342-345.

[24]In addition to those just cited, see also Stewart *op cit.;* E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 157-188; H. M. Halverson, The upper limit of auditory localization, *ibid.*, 38, 1927, 97-106; O. C. Trimble, The theory of sound localization: a restatement, *Psychol. Rev.*, 35, 1928, 515-523; L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-58. Boring (*op. cit.*) proposes further to reduce the temporal to an intensive difference. Trimble's more recent evidence stands against this possibility. O. C. Trimble, The relative rôles of the temporal and the intensive factors in sound localization, this JOURNAL, 41, 1929, 564-576.

[25]See, *e.g.*, M. Simpson, Experiments in binuaral phase difference effect with pure tones, *Phys. Rev.*, 15, 1920, 421-424; H. Banister, The effect of binaural phase differences on the localization of tones at various frequencies, *Brit. J. Psychol.*, 15, 1925, 280-307.

click as from a given direction. There finally remains the direct psychological attack upon the experiential changes which may accompany changes in phase, or changes in temporal relationships, in binaural hearing. Von Hornbostel[26] and Whittmann[27] have written extensive accounts of qualitative changes under these circumstances. Halverson[28] has attacked the problem quantitatively for the attribute of volume, while Banister[29] has worked with intensity. Since our own problem deals with these two and also with brightness and pitch, the results of these researches will be examined later.

(c) As the factors involved in auditory localization became better known, the evidence required some sort of systematization in terms of auditory theory. Much of the theorizing with localization has been done by recent advocates of non-resonance theories of hearing. The resonance theories were developed primarily to explain the facts of analysis.[30] Localization is for them entirely a secondary problem, the facts of which should be subsumed under the general theory with no more than a few minor supplements.[31] Theories starting from a different point of view may consider localization as of more importance.[32] A notable contribution in this direction is Boring's recent paper, which approaches theory from the facts of intensity, volume and localization rather than from pitch. Boring has shown that by envisaging audition from this aspect several equivocal interpretations of accepted experimental work could be offered, and a number of crucial experimental problems could be formulated. With respect to localization, Boring postulates (as did Watt) some sort of overlapping of the cortical areas of the two ears; and he then attempts to reduce the intensive, phasic, and temporal differences to a partial inhibition of the area of one ear by that of the previously or more intensively excited ear. Further details of these theories, which are related to the present problem, will be introduced and discussed after our facts have been presented.

The Attributive Character of Tones

A primary difficulty to be faced in any problem involving the experiential basis of sound is the lack of agreement among psychologists upon the auditory attributes. Within the last twenty-

[26] *Op. cit.*

[27] *Op. cit.*

[28] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this Journal. 33, 1922, 526-534.

[29] H. Banister, Phase effect and the localization of sound.

[30] *E.g.*, Helmholtz, *Sensations of Tone*, 1885; H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 11, 1921, 277-288; 12, 1921, 142-146, 362-382; C. A. Cosens & H. Hartridge, A vindication of the resonance hypothesis of audition (iv), *Brit. J. Psychol.*, 13, 1922, 48-51; (v), *ibid.*, 185-194; G. Wilkinson & A. A. Gray, *The Mechanism of the Cochlea*, Macmillan, 1924; H. Fletcher, *Speech and Hearing;* L. T. Troland, *op. cit.*

[31] See H. Banister, A suggestion towards a new hypothesis regarding the localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153; Fletcher, *op. cit.*, Troland, *op. cit.*

[32] See H. J. Watt, *The Psychology of Sound*, Cambridge Univ. Press, 1917, and A theory of binaural hearing, *Brit. J. Psychol.*, 11, 1920, 163-171; Boring, *op. cit.*

five years intensity seems to be the only attribute which has not been questioned.

Pitch, when considered as 'quality', received a blow from Watt[33] who looked upon it as an 'ordinal' attribute. Rich attempted to eradicate differences of opinion by studying the attributes experimentally, testing them for consistency and independent variability on the basis of their several limens.[34] He regarded volume as an attribute, and thought that it followed Weber's Law. Halverson later worked with volume, getting similar results.[35] Rich's observers were unable to differentiate brightness and pitch, but Abraham, employing the siren disk, found that he could vary brightness independently of pitch, and identified brightness with vocality.[36] Rich concluded that vocality was not an attribute. The theory that pure vowel sounds lie an octave apart[37] was not confirmed either by Rich[38] or by Weiss.[39]

Summing up the experimental literature to 1924, Ogden lists the attributes of sound as pitch, volume, brightness and intensity.[40] We shall provisionally accept this classification of the tonal attributes. Our specific problem is to determine what phenomenally represents a change in phase. A description in psychological language would be a description in terms of tonal attributes. The fact of disagreement among experimental psychologists regarding the auditory attributes suggests inherent difficulties in observation. Experimental reports dealing with one of the attributes of tone[41] may tend to confuse that one with others, unless the observers painstakingly acquire fixed standards of discrimination. In reviewing the studies on the tonal attributes the question often arises whether diverse terms do not sometimes mean the

[33]H. J. Watt, *The Psychology of Sound*, Chap. 2.

[34]Especially G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164. See also Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13-22.

[35]H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL, 13, 1922, 526-534, and Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360-367.

[36]O. Abraham, Töne und Vocale der Mundhöhle, *Zsch. f. Psychol.*, 74, 1915, 220-231, and Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

[37]W. Köhler, Akustische Untersuchungen (ii), *Zsch. f. Psychol.*, 58, 1911, 59-140.

[38]G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164.

[39]A. P. Weiss, The vowel character of fork tones, *ibid.*, 31, 1920, 166-193.

[40]R. M. Ogden, *Hearing*, Harcourt Brace, 1924. Ruckmick's recent classification ignores brightness (C. A. Ruckmick, A new classification of tonal attributes, *Psychol. Rev.*, 36, 1929, 172-180).

[41]*E.g.* Abraham's work with brightness, Halverson's with volume, Banister and other's with intensity. The physicists working with hearing usually handicap their careful technical control of the stimulating conditions by neglecting the training and instruction of their observers. They usually assume that pitch and intensity exhaust the character of pure tones.

same thing, and also whether the same term may not, at various times and for various observers, refer to different characteristics. It is therefore advisable, indeed necessary, to train observers accurately to discriminate the several aspects of tone, and carefully to describe as well as to label them.

Procedure, Apparatus and Preliminary Training

Our general course of procedure was first to train our *O*s in attributive description, secondly to confirm this description by obtaining liminal values, and thirdly to present in pairs successive tones produced by unlike phase at the two ears, instructing the *O*s to compare the two with regard to one of the attributes, pitch (P), volume (V), brightness (B), or intensity (I). Several vibrational frequencies were used in order that we might distinguish absolute temporal distance from relative phase-difference as a condition of localization and of attributive change.

The sound-source was an electric oscillating circuit which actuated a pair of Baldwin receiving phones (Fig. 1).[42] The phones were placed in wooden boxes and heavily padded with harness-felt. A small hole bored in one end of the box permitted the entrance of 6-mm. glass tubes. These tubes, which were five feet in length, fitted in turn into 9-mm. tubes of the same length. A thick coating of vaseline about the inner tube made the fit practically air-tight. From the ends of the large tubes rubber and glass connections led to two pairs (for two observers) of binaural caps. The phase of the wave-train was changed by shifting one or the other box together with its connecting tube in either direction along a steel centimeter-tape. By inserting an additional length of tubing between one phone and its fixed tubing the extent of a possible difference in phase was increased from about 130 cm. to twice that amount.[43] The boxes and tubes for changing phase were put in this parallel situation to effect greater differences of phase, and to afford fewer turns in the connections which might resonate or interfere with the transmission of the tones. As a matter of fact, the tones at the end of the binaurals were simpler and purer than the tones heard directly from the ear phones.[44]

[42]For the assembling and standardization of the apparatus the writers are heavily indebted to the technical knowledge, skill and good-will of Professor Elmer Culler.

[43]For a particular series the phones could be equated in intensity by changing their respective resistances. A test-series, however, with two observers instructed to report on intensity, in which both phones were moved in phase nearer to, and away from, the observer, showed that the differences in distance did not appreciably affect the intensity of the sound.

[44]Although oscillograms of the electric wave produced by the oscillator showed approximate sine curves, the vibrations at the telephone (especially with frequencies below 60 cycles) were not perfectly pendular. The Baldwin phone gives a much better tone, however, than the stock phone sold by the Western Electric Company. It was the best that we could command.

This method of changing phase was selected, despite Banister's criticisms,[45] because it allows a precise measurement of the extent of change in phase. Banister's results show that the angle of displacement for various observers was constant irrespective of pitch.[46] These results, which he himself discounts,[47]

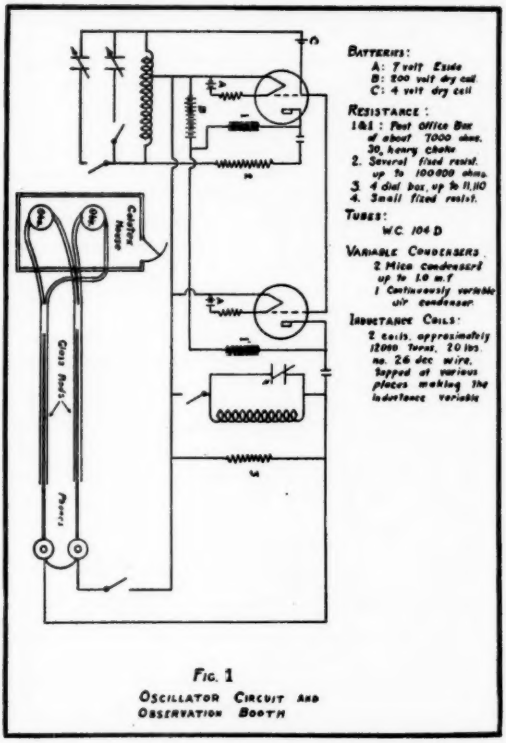
Batteries:
A: 7 volt Exide
B: 200 volt dry cell
C: 4 volt dry cell
Resistance:
1&1: Post Office Box of about 7000 ohms.
30 henry choke
2. Several fixed resist. up to 100000 ohms.
3. 4 dial box, up to 11,110
4. Small fixed resist.
Tubes:
WC 104 D
Variable Condensers
2 Mica condensers up to 1.0 m.f
1 Continuously variable air condenser
Inductance Coils:
2 coils, approximately 12000 Turns, 20 lbs no. 26 dec wire, tapped at various places making the inductance variable
Cathode Ray Tube
Glass Rods
Mirrors

FIG. 1
OSCILLATOR CIRCUIT AND OBSERVATION BOOTH

were doubtless due to the fact that his observers compared only 180° differences with the standard of 0°. The dual tones which Whittmann observed, like the buzz in Wilson and Myers' report, may be due to the resonance or filter effects of his U-shaped pipes.[48]

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[45]H. Banister, The effect of binaural phase differences on the localization of tones at various frequencies, *Brit. J. Psychol.*, 15, 1925, 280-307.

[46]*Op. cit.*

[47]H. Banister, A suggestion towards a new hypothesis regarding the localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153.

[48]Whittmann, *op. cit.*

The observers sat in a 'Celotex' house, 6 ft x 6 ft x 3 ft, which allowed the simultaneous observation of two subjects and served to cut off visual distractions. It also permitted the experimenter to work in the same room. The reports were made on mimeographed blank-sheets. The phone-circuit was cut with a knife switch, and the duration of the stimulus was controlled by a silent pendulum. The compared tones were about 1½ sec. long and separated by a 1-sec. interval.

The method of constant stimuli was used with five (or sometimes seven) comparison-positions. Half the judgments in each series were based on pairs in which the standard was first, and in half the comparison stimulus was first. The phones were alternated from left to right side in order to factor out whatever differences in quality they might condition. The rubber and glass tubing was so arranged that a given phase was presented to opposed ears of the two observers, who sat alternately on left and right within the observation-house. No observations were made on this apparatus with changes in phase, however, until after extensive preliminary training had been carried out under the various instructions for P, V, B and I.

Most of the training consisted in making reports on pairs of tones produced by Stern variators. The University air-supply, with a pressure of 5 atmospheres, was controlled by a system of needle-valves. Minor unevennesses in the supply were smoothed by inserting a metal air-reservoir in the line. A water-manometer graduated in centimeters was kept constantly in the circuit of the variator.

These preliminary observations were made by several observers at once and in the room with the variators. The judgments took the form of written reports on mimeographed blanks. The tone from variator No. 1 was interrupted by releasing an escape-valve (thumb over open end of Y tube in circuit), which dropped the pressure below that necessary to produce a tone; the other variators were interrupted by turning the air-jet suddenly away from the mouth of the bottle. Frequencies of 150 (variator No. 1, pressure 7.5 cm.), 550 (variator No. 4, pressure 2 cm.), and 900 (variator No. 6, pressure 3.5 cm.) were used as standards. The comparison-intervals were first made very large and then cut down step by step to suit the discrimination of the observers. On the variators there were in all for each observer about 600 reports on P, 600 on B, and about 850 on V and 850 on I. A device for changing B without changing P was made[49] by cutting 8 concentric rows of 20 circular holes of like diameter in a cardboard disk. A blast of air on the various rows of the rotating disk gave tones of like P but different B. A demonstration from this apparatus preceded every hour's work on B throughout the course of the experiment. For demonstrating to the Os intensive differences, the energy was changed by a slight variation in the air-pressure upon the variator. A few verbal reports were taken on pairs of such variator intensities (at various frequencies) prior to each set of observations upon intensity.

The Liminal Determinations

The limens for the various attributes were not determined immediately upon the conclusion of the preliminary series but

[49] O. Abraham, Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

just before the reports were taken upon a given attribute with variation of phase (see Table I). The limens were determined at the frequencies of 150 and 500 by the constant method, using 5 stimuli from the variators. Again an hour or two of practice was required before the limens were determined.

TABLE I

LIMENS (IN VIBRATIONS) FOR P, V, AND B, RECKONED FROM X_1 (SUBJECTIVE EQUALITY)

		Pitch Limens				Volume Limens				Brightness Limens			
		150 dv		550 dv		150 dv		550 dv		150 dv		550 dv	
Obs.		High	Low	High	Low	Less	More	Less	More	Brt.	Dull	Brt.	Dull
B	DL	.31	.24	.89	.89	2.19	.80	1.43	.62	1.18	1.64	3.32	3.73
	h	.55	.72	.56	.56	.15	.37	.12	.27	.43	.31	.09	.08
C	DL	.36	.30	.47	.53	2.06	1.45	5.04	5.10	1.32	1.53	3.91	3.09
	h	.74	.88	.46	.40	.27	.38	.15	.15	.87	.75	.11	.24
F	DL	.40	.26	.29	.29	.62	.34	.63	.13	.28	.37	1.10	1.35
	h	.79	1.22	.30	.30	.83	1.58	.32	1.64	.97	.73	.36	.29
G	DL	.37	.48	.36	.54	1.51	.27	.86	.78	.96	.62	1.44	1.17
	h	1.27	1.06	.65	.43	.71	3.95	.25	.27	.39	.61	.16	.19
H	DL	.26	.20	.54	.54	1.11	2.01	2.62	2.86	.54	.42	1.68	1.20
	h	.90	1.20	.25	.25	.56	.31	.08	.08	.25	.33	.11	.16
I	DL	1.17	.32	.96	1.16	.74	1.11	3.02	3.45	.90	.88	2.26	3.94
	h	.11	.40	.30	.25	.35	.25	.08	.07	.44	.45	.26	.15
Mc	DL	1.23	.65	1.49	1.98	3.28	2.18	9.34	3.84	1.44	1.74	3.56	3.56
	h	.59	1.12	.73	.53	.12	.18	.10	.24	.70	.58	.22	.22
M	DL	.07	.06	.29	.59	.13	.11	.33	.57	.78	.64	2.20	1.14
	h	3.06	3.81	1.21	.60	1.00	1.22	.92	.53	.59	.71	.13	.25
S	DL	.48	.52	1.60	1.22	1.22	.96	2.44	3.41	1.08	1.52	2.64	3.08
	h	1.01	.94	.36	.47	.42	.47	.18	.13	.44	.31	.27	.23
Z	DL	.40	.41	.53	.41	.62	.83	1.47	.89	no limen computable			
	h	1.21	1.20	.48	.63	.72	.85	1.05	1.64				
Av. Limens		0.505	0.344	0.742	0.815	1.348	1.006	2.718	2.165	0.94	1.04	2.46	2.47

The instructions during the entire experiment followed this general form (given in an example for V).

"After a 'ready' signal there will be two tones. Compare these with respect solely to VOLUME. If they are the same in VOLUME record "S." If they are different ("D") indicate, if you can, whether the second tone is greater or less than the first. A statement of the criteria will be requested at the end of the series."[50]

The intervals used for the comparison-stimuli at frequencies of 150 and 550 were; for Pitch 0.5 dv (at 150) and 1.0 dv (at 550); for Volume, 2.0 dv and 3.0 dv (except for the group G, F, M, and Z, for whom the intervals were gradually

[50]The observers, to whom the writers are indebted for many long hours of service, are as follows. (1) Those with musical training and with experience in related auditory researches: Dr. Sullivan (S) (Observer for Bishop); Dr. Guilford (G) (Observer for Beck); Dr. Möller (M) (Observer for Bishop, Halverson and Möller); and Mr. Macdonald (Mc) Observer for Pratt, Möller, Halverson and Macdonald). (2) Observers with musical training only; Mr. Zuschke (Z) and Mr. Fox (F). (3) Observers with limited musical training: Dr. Culler (C), Dr. Higginson (H), Mr. Bidwell (B) and Miss Ireland (I). The last two were graduate students. The others were members of the departmental staff.

reduced on 3 successive days to 0.5 and 1.0); for Brightness, 1 dv and 3 dv; and for Intensity, 2.0 dv and 3.0 dv. The limens are based on 20 reports for each comparison. The time-orders were half sv and half vs.

The limens and measures of precision were computed by the use of Urban's tables. The figures (Table I) give the limens (DL) from the point of subjective equality, Xi,[51] together with the measures of precision (h) for P, V and B.

It may be observed that the limens for volume are lower as a group than those obtained by Rich[52] or Halverson;[53] although the determinations for one of Rich's observers, Professor Ogden, fall well within the range of our figures. Furthermore, there is little difference between the average limens for V and B with these ten observers.

Although the P, V, and B series served as training, the I series had a theoretical bearing. According to Boring's theorizing, V does not necessarily change with P, nor do V and I change simply with the energy of the stimulus; both vary with the amplitude of the vibrations.[54] He thus suggests that the V-limens obtained by Rich are artifacts of the experiment, due, not to the ostensible change in frequency, but to fortuitous changes in amplitude which would occur if the energy remained constant. Boring points to Halverson's work on V and I[55] as showing the more fundamental relationship. Thus, if we work out Boring's hypothesis,[56] we find it possible to determine a limen for I of about 20 dv at 400 cycles, and, since the V-limen is about twice as great as the intensive limen, it should be considerably higher at this

[51]E. Culler, Studies in psychometric theory, *Psychol. Monog.*, 35. (No. 163), 1926, 71-75.

[52]Rich, *opp. citt.*

[53]Halverson, *opp. citt.*

[54]Boring, *op. cit.*

[55]H. M. Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360-367.

[56]We are told that, pitch constant, the limen for V is considerably more than that for I (Halverson, *op. cit.*); and that, energy constant, the limen for V at a frequency about 500 dv is approximately 12 dv (Rich). It follows that when energy is constant an I-limen should likewise be found, less than the V-limen. The extent of such a limen may be computed from the results of Knudsen, who has determined over a wide range of frequencies the proportional increment in pressure which is perceived as being just different (V. O. Knudsen, Sensibility of the ear to small differences in intensity and frequency, *Phys. Rev.*, 21, 1923, 84-102). At 400 dv the increment is 0.115. Now, since energy is proportional to the product of the squares of the amplitude and frequency, the increase in the amplitude would be .056. With energy remaining constant, a change in the amplitude by the amount 0.056 would increase the frequency about 22 dv (it would decrease it about 17 dv).

frequency. As can be observed from Table II, no such I-limen could be found for the 9 *Os* in this experiment; and no V-limen has been found much larger than 15 dv at this frequency.

TABLE II

REPORTS ON INTENSITY WITH CHANGE OF PITCH (IN PERCENTAGES). METHOD OF CONSTANT STIMULI

	Obs. in Group 1				Obs. in Group 2					
	B		Mc		C		H		S	
Freq.	+	−	+	−	+	−	+	−	+	−
154	70	20	5	80	100	0	80	0	55	5
152	45	30	0	25	80	0	35	0	0	55
Stand	10	30	10	0	15	10	10	10	30	10
148	30	50	30	5	0	95	0	60	25	35
146	85	15	45	0	0	95	0	75	50	15
556	20	75	70	20	90	0	55	20	70	0
553	30	60	35	5	75	15	30	35	15	10
Stand	25	5	5	0	10	0	0	10	5	15
547	50	40	20	30	20	70	15	60	15	25
544	15	80	10	65	0	90	5	90	15	15

	Obs. in Group 3							
	F		G		M		Z	
Freq.	+	−	+	−	+	−	+	−
154	90	0	65	0	35	40	5	90
152	70	10	65	20	30	40	0	70
Stand	20	30	40	35	35	15	30	5
148	0	100	30	70	10	40	15	20
146	50	25	100	0	10	85	0	45
556	20	55	20	25	5	70	30	10
553	65	15	10	60	35	50	10	0
Stand	0	45	30	45	20	25	5	0
547	100	0	80	0	45	35	90	10
544	90	0	15	35	60	10	15	0

THE RESULTS WITH PHASE DIFFERENCE

The frequencies used in all the determinations with phase-difference were 100, 400 and 1000 cycles, produced by the oscillator. Most of the results from the four attributive instructions (P, V, B and I) are based upon the same settings of comparison and standard. The speed of sound for the normal room-temperature was estimated to be around 34482 cm. per sec. in the open air and somewhat less in the tubes. Half the wave length ($\lambda/2$) for a frequency of 1000 cycles would then be slightly greater than 17 cm.

Three positions of the phones were used as comparison-values for 1000 cycles, the distances between them being 0, 8.5 and 17 cm. The standard, 0, occurred first in half the determinations

and second in half. At a frequency of 400 cycles, $\lambda/2$ would exceed 40 cm, a distance which was divided into five positions, the comparison distances between the phones being 0, 10, 20, 30 and 40 cm., respectively. Two different positions were used as standards, so that the area would be explored twice, the 0 and 20 cm. settings being thus used. The sequence of standard and variable was again, as in the other series, alternated at random so that the observer would never be reporting only upon differences of phase in the same direction. For the frequency of 100, $\lambda/2$ would approximate 172 cm. It would take sound approximately .005 sec. to travel that distance, a duration well over the upper critical time for localization based on time-differences.[57] For the purpose of a direct comparison, the first 40 cm. of this distance was divided, as was the 40 cm. of the 400-cycle frequency, into 10-cm. steps with standards at the 0 and 20 cm. settings. The next 120-cm. difference was divided into 15 cm. steps, the mid-points of each successive five (70 cm. and 130 cm.) being chosen as standards. Thus only a 160-cm. phase-difference ($\lambda/2 = 172$) was used.

After a long series of preliminary trials upon *volume*, a main series of observations was obtained at 1000 dv. with three constant stimuli, and at 400 dv. and 100 dv. with five constant stimuli. The results of several thousand observations (Table III) gave considerable evidence of change of volume with variation of phase. But the character of the volumic change ('greater' or 'less') was not consistent with the direction (increase or decrease). A possible total of 10 or 11 different limens can be computed for each of the 6 individuals who made the final observations. There are actually only 38 cases where the percentage of judgments of greater or less equal or exceed 50% of the judgments at that comparison value. These are distributed as follows among the several observers.

	Less volume	More volume	Neither
B	4	2	4
C	8	1	2
F	2	5	4
G	1	7	3
M	5	3	3
Mc	0	0	10
Totals	20	18	26

[57] O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Arch. f.d. ges. Psychol.*, 40, 1920, 117-146; E. M. von Hornbostel & M. Wertheimer, Ueber die Wahrnehmung der Schallrichtung, *Sitzber. Akad. preuss. Wiss.*, 1920, 388-396.

The true phenomenal basis for this result is not clear. We can only append the following comments. *Os* complained of the uncertainty of 'volume' in this curious and baffling situation

TABLE III

REPORTS ON VOLUME WITH CHANGE OF PHASE
(S = Standard stimulus)

	Phase Diffce	Obs C Great	Obs C Less	Obs G Great	Obs G Less	Obs Mc Great	Obs Mc Less	Obs M Great	Obs M Less
				Tone of 400 dv					
S	0	0	0	5	20	0	5	3	15
	10	30	10	55	5	5	15	33	45
	20	35	30	80	0	25	10	58	38
	30	20	50	70	15	15	15	40	53
	40	35	30	45	30	5	30	33	63
	0	55	5	20	50	25	20	50	45
	10	30	0	20	35	10	10	50	40
S	20	5	0	20	25	0	0	10	20
	30	0	65	30	25	10	10	13	55
	40	15	50	45	30	10	10	20	55
				Tone of 100 dv					
S	0	10	15	10	5	5	0	10	10
	10	20	50	55	0	5	0	30	25
	20	25	40	85	5	15	10	60	15
	30	30	45	90	0	20	30	65	25
	40	35	40	90	0	30	20	70	25
	0	40	25	60	40	40	15	35	50
	10	45	15	25	35	15	0	40	30
S	20	15	10	35	35	5	0	10	15
	30	10	35	40	40	5	5	25	30
	40	25	30	40	30	35	15	50	25
	40	50	20	50	40	35	10	75	10
	55	25	35	15	55	0	5	20	20
S	70	5	10	10	25	5	0	5	10
	85	35	30	65	25	5	10	30	65
	100	55	15	55	10	20	35	25	45
	100	80	5	25	50	5	10	35	45
	115	50	30	20	50	0	5	40	25
S	130	15	15	20	10	10	0	20	15
	145	0	55	35	35	20	0	25	40
	160	0	95	60	15	30	0	30	35

where the tone was constantly moving from ear to ear in its apparent course from side to side and back toward the median plane of the head. The criteria for 'volume' seemed shifting and uncertain. The general characterization of the attribute was an aspect of 'boundary' or 'extent' of the tone; but B and M thought volume not 'simple,' nearly all of the *Os* declaring at the end of the ex-

periments that volume and brightness were intimately related (6 of them said 'inversely related'), and several commented on volume's subtle dependence upon both pitch and intensity, attributes which were subjected, in our experiments, to wide variability in binaural tuning and acuteness. All *O*s found it extremely difficult to abstract from other changes induced by the experimental setting.

TABLE IV

REPORTS ON PITCH WITH CHANGE OF PHASE

(S = Standard stimulus)

	Phase Diffce	Obs. F Great	Obs. F Less	Obs. C Great	Obs. C Less	Obs. G Great	Obs. G Less	Obs. M Great	Obs. M Less
				Tone of 400 dv					
S	0	10	40	20	35	0	5	5	5
	10	30	50	25	40	85	0	35	50
	20	5	80	45	20	95	0	35	55
	30	10	70	15	35	85	0	75	25
	40	50	30	40	25	90	0	50	40
	0	60	25	70	10	60	10	65	25
	10	45	40	55	20	55	15	45	45
S	20	15	40	10	15	5	10	20	10
	30	60	20	45	35	10	25	20	70
	40	65	15	55	25	65	10	25	65
				Tone of 100 dv					
S	0	35	30	5	10	0	0	5	20
	10	35	35	15	25	60	0	30	50
	20	45	35	20	65	80	0	30	55
	30	45	50	5	70	90	0	35	55
	40	60	35	0	80	95	0	45	40
	0	25	65	50	10	75	5	45	55
	10	20	55	65	10	75	10	40	50
S	20	45	20	10	45	5	15	25	5
	30	70	20	15	60	15	35	20	35
	40	55	30	15	65	5	85	50	30

The reports upon *pitch* take a course similar to that for volume. The 'different' reports under phase-difference run even higher than in the case of volume. But again the direction and the amount of pitch-change do not run with the direction and the amount of phase-difference, and, as before, any standard when compared with itself gives most 'equal' reports of all. The reports for F, C, G and M at four of the seven settings are given in Table IV.

A complicating factor here is the 'pitch difference' of the ears. G, for example, reported 'lower' when the localization was on the right, and 'higher' on the left. When allowance is made for this difference of tuning, our results give no clear evidence of a direc-

tional change in pitch with a phasic modification of the wave-train. The difficulties of isolating a pitch-change are illustrated by the following remarks from the commentaries of this set.

C. "The two tones usually differ in some respect (the second being localized to one side); but I am not sure wherein the difference consists. It seems P sometimes, but usually not. Sometimes a change in B apparently."
S. "Localization distracts; judgments often based on P-B."
H. (preliminary) "I begin to doubt P-differences, and think perhaps it is V."
M. "I am not at all sure of my P-judgments."
G. "I don't believe there are many P-differences. Perhaps they are all equal!"

We had expected the reports upon *intensity* to be simple and straightforward; but they were the most confused and inconsistent of all. Here it was obvious to the Os that whatever change the sounds did undergo with phasic modification made a comparison of intensities extremely difficult. Here are relevant comments:

C. "There is always a tendency to establish a correlation between localization and the attributes in question, and to judge by inference when the criteria fail." "I is the product of density and volume, and the total weight of the tone." "This set seems to be entirely guesswork, there are so many differences in P, V and B involved."
M. "I think I am really judging a combination of other things. The B-difference has a lot to do with it."
Z. "Changes in B seem to veil the intensity aspect." "The second tone always seems larger, and the longer it lasts the more intense it seems." "Judgments are best when the series is a, b, a." "When I observe intensity, I can get brightness nicely!"

The pairs of unlike phase returned from 15% to 45% of 'greater' or 'less' reports; but the standard when compared with itself also gave a surprising number (up to 27%) of the same kind. While there was a distinct tendency to increase the 'different' reports with increase of interval reckoned from the standard, that increase was as apparent when the interval was directed toward like-phase (*e.g.* the phase-series 0, 10, 20, 30 and 40 cm. with the standard at 20 cm.) as when the series of comparisons moved out from like-phase toward a great difference (*e.g.* the same series, 0, 10, 20, 30 and 40 cm, with the standard at 0). This fact is apparent in the averages for all observers in these series at 400 dv.

AVERAGE OF REPORTS ON INTENSITY (400 dv)

Phase Diffce		Greater	Less	Total		Greater	Less	Total
0	Stand = 0	14	14	28		25	48	73
10		30	23	53		18	35	53
20		43	25	68	Stand = 20	16	27	43
30		31	40	71		23	41	64
40		47	28	75		42	29	71

With all these attributive comparisons, then, for volume, pitch, and intensity, we find virtually the same thing, *i.e.* a general increase of 'different' reports in comparisons moving away in both directions from the standard, but with no consistent difference in the apprehended direction of 'greater' and 'less,' 'higher' and 'lower,' to accord with the modification of phase.

The series for *brightness* wears a different complexion. Individual results for 5 *O*s and average calculated from all *O*s are given below (in percentages). Here appear (1) a small number

TABLE V

REPORTS ON BRIGHTNESS WITH A CHANGE IN PHASE
(S = Standard stimulus)

	Phase Diffce	Obs. B		Obs. C		Obs. F		Obs. G		Obs. M		Av. 10 *Os*	
		Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less
						Tone of 400 dv							
S	0	10	10	5	0	15	10	20	35	0	0	7	9
	10	25	55	10	55	10	65	10	65	15	70	14	44
	20	45	45	5	80	15	85	0	85	15	85	19	56
	30	20	80	0	75	20	65	5	90	25	80	14	70
	40	35	60	0	100	5	90	20	75	10	90	18	68
	0	50	30	73	15	70	15	70	15	65	25	60	17
	10	50	20	53	13	20	45	50	30	60	15	44	22
S	20	10	20	13	23	20	35	40	10	25	30	17	18
	30	30	30	5	33	40	50	35	50	20	55	23	40
	40	10	80	23	55	25	70	25	70	25	70	20	58
						Tone of 100 dv							
S	0	20	30	0	10	0	5	5	25	0	0	5	10
	10	0	50	5	35	0	70	0	95	0	80	8	46
	20	0	85	5	50	10	80	0	100	5	90	10	64
	30	0	100	10	65	0	90	0	85	0	100	5	71
	40	5	95	0	75	20	80	0	90	0	95	7	74
	0	55	30	85	0	70	10	53	25	50	25	55	23
	10	50	25	55	15	40	30	43	18	35	25	34	22
S	20	30	25	15	10	0	35	15	20	15	10	15	17
	30	30	35	20	30	30	15	18	43	15	55	24	31
	40	20	65	25	50	25	50	18	48	15	80	21	48
	40	85	0	50	20	70	5	55	5	55	35	54	16
	55	45	25	40	25	40	20	35	15	50	25	34	20
S	70	40	15	30	40	5	30	25	5	20	5	20	15
	85	30	45	10	60	5	30	30	40	25	50	19	33
	100	10	75	5	75	10	45	5	60	20	60	14	50
	100	30	50	55	30	45	5	43	28	50	15	47	16
	115	10	50	45	35	20	40	28	38	15	40	23	35
S	130	30	30	35	20	10	30	30	23	25	20	19	17
	145	30	40	35	45	35	35	48	13	35	30	32	25
	160	35	35	75	15	60	15	68	15	65	10	51	20
						Tone of 1000 dv							
S	0	35	10	0	40	0	10	25	10	10	10	12	12
	8	40	35	3	63	48	45	20	28	33	40	26	39
	16	33	60	5	68	50	50	10	63	28	60	23	56

of 'different' reports upon the standard pairs, equally divided between + and −, (2) an increasing number of 'different' reports as the phase-difference moves in opposed directions from the standard pair, (3) a decided increase in 'brighter' reports as like phase is approached and of 'duller' (less bright) reports as phase difference is increased. The commentaries showed that the Os H, I, Mc, S and Z were uncertain (through a part of the experiments) of their criteria for brightness; though all came in time to recognize it from instruction with the siren-disk. While their results bear out the above rules, the figures are clouded by a shifting of the basis of report. But the other 5 Os (B, C, F, G, and M), all of whom carried fixed criteria for brightness, exemplify the three rules throughout their reports, except for a reversal at the greatest phase differences (130 to 160 cm) of the tone 100 dv. The region around $\lambda/2$ needs further exploration.

Resuming the observations upon attributive character, then, it appears that, under our conditions and in the tonal regions which we examined, brightness alone (of the four attributes) undergoes a consistent change with change of phase and that the rule is for tones to grow duller and duller in dichotic hearing as their right and left components depart farther and farther from like phase.

After the above experimental program (which included about 7000 reports from each O) had been carried out, each O had placed before him for answer the following questions. Before his answers were required, he was presented over and over with the tones 400 dv and 100 dv, given dichotically in successive pairs as in the regular experiments. The pairs contained one member at like phase and the other at a large phase-difference (40 cm. or more upon one tube).

The questions were as follows:

1. Describe P, V ,B, and I in terms of auditory experience.
2. What, descriptively, are their inter-relations and dependences?
3. Do you think that these attributes fairly exhaust the description of a simple tone?
4. Do you think that they constitute distinct aspects or ways of regarding the tone?
5. How certainly can you distinguish changes in them with a change in phase?
6. Which of them change most prominently with a change in phase?
7. What is the direction of this change?
8. In which series were the differences in localization most disturbing?
9. Describe the experiential differences in localization with a change in phase.

The first question was designed to orient the *O*. The first four strike directly at the attributes. Questions 5, 6 and 7 refer to changes accompanying phase-difference. Question 8 was put in to determine whether any of the observers might inadvertently be reporting localization rather than attributive difference; and the last question directly bears upon the main problem of the entire experiment.

O was instructed to search in turn for one attribute after the other at each new setting of the oscillator and to ask for a repetition of the pairs until he was thoroughly satisfied as to his answer for each attribute. The answers, which were written out, are given below in abstract form.

1. Description of tonal attributes
   - A. Pitch
     - i. the aspect which varies up and down (B, F, I, Mc, M, S, Z)
     - ii. the distinguishing qualitative attribute (G)
       pitch comes as size (H)
       high tones are smoother, clearer, thinner, denser, compacter (C)
   - B. Volume
     - i. an aspect of the boundaries or extent (C, F, G, H, I, Mc, M, S, Z)
       seems a composite; dependent upon brightness (B)
     - ii. It is also a matter of filling (M, S)
   - C. Brightness
     - i. an aspect of the filling, or clearness of emergence (B, C, F, G, H, I, Mc)
     - ii. the glint or glitter which attaches to V of tones (M, S)
       antithesis between veiled and non-veiled, tubercular and healthy, etc. (Z)
   - D. Intensity
     - i. the aspect of force or strength (all but G)
     - ii. the total mass or impressiveness of the tone (G)
2. Interrelations and dependences of the attributes
   - i. B and V are inversely related (B, C, F, G, I, M)
     (P and I may be involved)
   - ii. P changes at least B and V (all Os)
   - iii. I is greater with high tones (C, H)
   - iv. I varies with V (G, I, M, B? F?)
3. They exhaust the description of a simple tone
   - i. Yes (F, Mc, S) Yet they are not simple (M)
   - ii. They overlap, V being complex (B, I)
   - iii. They leave out that total impression, the *simple* tone (H, M, Z)
   - iv. Should add "density" (C, G)
4. Are they distinct aspects or ways of regarding tones?
   - i. Yes; but cannot be separated (B, F, G, H, Mc, M, Z)
   - ii. Brightness is not (S)
   - iii. Not sure about V and B (I)
   - iv. P and I seem fundamental, B dependent upon the purity of the tone, while V depends on some optical feature (C)
5. How certain are judgments with phase changes?
   - i. Not certain (G, H, I, Mc, M)
   - ii. Certain in the order B V P I (B)
     I B P V (F)
     I P V B (S)
     P B I V (Z)

6 & 7. How do the attributes change with increase of phase-difference?
- i. B decreases (all but S)
- ii. V increases (B, G, Mc, F?), decreases (H), direction doubtful (C, Z)
- iii. P increases (F), decreases (C, H)
  Low on right; high on left (G)
- iv. I increases (F?), decreases (C, H, Mc)
  More on right, less on left (G, M)

8. In which series did localization hinder?
- i. In P (H, I, S)
- ii. In V (C, F, G, I)
- iii. In B (C, F, I, Mc)
- iv. In I (Mc)
- v. Assisted in V and B (B)

9. What experiential differences in localization accompany a change in phase?
- i. hear "tones in one or the other ear" (B, F, H, Mc, M, Z)
- ii. some tones inside head (B, C, F, G, H, M, Z)
- iii. shifts from one side to the other (I, M, S)

These answers we take into account in our following discussion and interpretation of the experimental results.

Discussion

The Tonal Attributes. P and I remain unquestioned as attributes. The reports on B, both in the training and liminal series and with changes in phase, indicate that, as Köhler,[58] Abraham,[59] Weiss,[60] Ogden,[61] and others maintain, B is an attribute. We fail to get quantitative results confirming the previous work on V.[62] Perhaps it would be well if we could throw V out, as Banister has desired.[63] He contends that it is sounding objects which we perceive, and not sounds; that the conception of V has arisen (a) from our tendency to objectify our experiences, and (b) from a confusion of multiple locations of the sounding source. This assumption seems scarcely to accord with the phenomenal facts. Moreover, V has been identified as an attribute in at least three situations not involving localization: as a co-variant of P and of I[64] and (in the present experiment) of B. The relation of V

[58]W. Köhler, *op. cit.*

[59]O. Abraham, Töne und Vocale der Mundhöhle, *Zsch. f. Psychol.*, 74, 1915, 220-231; and Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

[60]A. P. Weiss, The vowel character of fork tones, this Journal, 31, 1920, 166-193.

[61]R. M. Ogden, *Hearing.*

[62]G. J. Rich, *opp. citt.*; H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this Journal, 33, 1922, 526-534, and Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360-367.

[63]H. Banister, Auditory theory; a criticism of Professor Boring's hypothesis, this Journal, 38, 1927, 436-440.

[64]See Rich, *opp. citt.* and Halverson, *opp. citt.*

to B has appeared in our experiments to be especially intimate. V has been reported both as identical with B and as varying inversely to it.[65]

The Attributes and Phase. As we have said, B is the only property of the tone to vary consistently under change of phase. No one has (so far as we know) found a directional change of P with modification of phase and we have certainly found no positive evidence for such a correlation. Fixed differences in binaural tuning should, however, be taken more strictly into account before a final decision is reached. Intensive differences with changes in phase have been several times reported. Thompson[66] reported intensity as increasing with lateral shifts and phasic displacements. According to Bowlker and to the Wilson-Myers hypothesis[67] the intensity of the lateral tones should be greater than median tones. Rostosky[68] and Stewart[69] report the median tone as most intense, but discovered a pair of secondary maxima just short of $\lambda/2$. Von Hornbostel maintains that lateral tones, though fuller, are less intense than medial.[70] Halverson concluded that there were intensive differences, but was uncertain as to their direction.[71] More and Fry, however, with 14 observers looking especially for intensive differences, concluded that there were none.[72] Most systematic and careful was Banister's recent study.[73] He presented at random to his Os both intensive and phasic differences (comparing binaurally equal phase with $\lambda/2$). At first the Os reported the phase change as louder. With practice, however, they decided that the change was not one of intensity, but one of quality (brightness?). Our own negative

[65]See also R. H. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.

[66]S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, and Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391.

[67]T. J. Bowlker, *op. cit.;* Wilson & Myers, *opp. citt.*

[68]P. Rostosky, Ueber binaurale Schwebungen, *Phil. Stud.*, 19, 1902, 557-598.

[69]G. W. Stewart, Binaural beats, *Phys. Rev.*, 9, 1917, 502-508, The secondary intensity maxima in binaural beats, *Phys. Rev.*, 9, 1917, 509-513, The theory of binaural beats, *Phys. Rev.*, 9, 1917, 514-528, and Binaural beats, *Psychol. Monog.*, 25, (No. 108), 1918, 31-46.

[70]E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[71]H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL, 33, 1922, 526-534.

[72]L. T. More & H. S. Fry, On the appreciation of differences of phase of sound waves, *Phil. Mag.*, (6) 13, 1907, 452-459.

[73]H. Banister, Phase effect and the localization of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431.

results seem to suggest that observed differences may be due to a confusion of the attributes and to unlike sensitivity of the two ears.

The relations of V and phase have been explicitly studied elsewhere only by Halverson. His own accounts conflict. In one report he notes that lateral tones are more diffuse, voluminous and intense.[74] In a later quantitative study of V and phase, however, he obtained that curious curve which caused Boring some difficulty in explaining, where V decreased for a time and then again gradually increased. But for a lateral tone, contrary to his previous report, V was less than for a medial tone.[75] It may be noted that one of Halverson's *O*s (Mc) sat in our experiments. Mc wrote in his final questionary that V increases with a change in phase; but neither his reports nor the reports of our other Os lend statistical support to this statement; unless it should turn out that V may be identified, as an attribute, with B.

There exists a certain plausibility for the attachment of B to phase. The striking B-differences upon the siren disk with a single frequency suggest that the deformation of wave-form from temporally varying interruptions upon the disk is here responsible.[76] The phase-difference in our experiments would seem to produce a similar deformation. If the wave-trains were wholly dichotic (as we have assumed), then the deformation would have to be centrally represented (*e.g.* by the hypothetical 'overlapping' of cortical fields which we have discussed, or, possibly, at subcortical levels).

Just what relation obtains between the brightness variable with the siren disk and with increase in frequency (the higher tones are commonly regarded as brighter) no one, as far as we know, has ascertained. It may be deformation again. Whether brightness-change, as such, acquires in experience a spatial (right-left) coefficient which is read off as localization is worth

[74]H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, this JOURNAL, 33, 1922, 187.

[75]H. M. Halverson, Diotic tonal volumes as a function of difference of phase, *ibid.*, 33, 1922, 526-534.

[76]Clear brightness differences also appear when episkotister disks with radial sectors are substituted for the concentric holes. The only physical differences at various radial distances upon the episkotister would seem to be the absolute time-gradient at onset and cut-off and air-displacement under changing velocity (see Gundlach, *op. cit.*). This matter, as well as the 'brightness' of high tones and the 'dullness' of low, is being further investigated.

considering.[77] The obvious brightness-difference of vocal sounds also suggest that both the (brighter) overtones of the higher ranges and the several formants which appear to rest upon deformed wave-trains may be involved. The sharp localization of these configurations of tone, formant and noise may be intimately related to these facts of brightness.

The phenomenal similarity between a tone laterally localized and the same tone produced through one ear was exhibited when we slipped off the tube carrying the leading phase and *O* reported only a more lateral localization. The loss and subsequent reentry of this tone (when the tube was again connected) were not observed by *O*, who experienced only a change in lateral placement of the sound. One observer was surprised to hear that all tones in the regular series had had both right and left components. He had supposed that large lateral displacements came from a monaural source. These facts tend to sustain the theory that the prior entry of the sound at one ear (leading in phase) inhibits the full cortical effect of the other.[78]

Summary and Conclusions

This article reports preliminary experiments upon the descriptive character of tones and upon certain conditions under which this descriptive character varies. The special condition here considered is alteration of phase under binaural stimulation. The study seemed appropriate here and now both because it was a logical step in auditory research and because we were able to command several observers who had taken part in similar experimental studies conducted elsewhere, studies which have made

[77] We hope that subsequent experiments comparing the relative rates of brightness-change and of local place may throw some light here. We also hold to the possibility that tactual impressions upon the tympanic membrane may play a part.

[78] The fact that phase-distance could be increased, in our experiments, above 170 cm (=.005 sec. for the rate 100 dv) without producing a dual sound, with a temporal order, confirms Whittmann's criticism (*op. cit.*) of Klemm (*Arch. f.d. ges. Psychol.*, 38, 1918, 71-114) and of von Hornbostel and Wertheimer (*op. cit.*), who set the greatest limit for the perception of a single click at about .002 sec. Whittmann finds the optimal to be .0026 sec, and the greatest difference near .016 sec. In our experiments no significant changes occur around the critical times set by any of these researches; the only significant change being the uncertainty of B-change beyond 100 cm. The discrepancies may be due to errors in the procedure by the various experimenters or possibly to an inherent difference between the localization of tones and of clicks. The reduction of localization based on phase-differences to time-differences, however, seems justified.

distinct contributions toward our present conception of tonal character. We first assumed that the current descriptions in terms of tonal attributes, based upon several careful researches, were sufficiently seasoned for our case; but the outcome of the experiments casts doubt upon this assumption. An exhaustive description of tones in attributive terms is exceedingly difficult. In the comparison of unlike tones in terms of a single attribute, pitch and intensity are most unequivocal and most easily reported upon. Brightness and volume are more equivocal and more difficult of independent variation and report. They appear to be related, in a complicated way, to pitch and intensity. The best means that we could find to define and to identify brightness was to refer it to that attributive variation which prominently appears when tones of like rate but of variable interruption (as from concentric openings) are sounded upon the siren disk.

The difficulties of attributive comparison of tones are greatly increased when phase-difference and consequent shifts in localization are introduced. Here all the attributes are compared with difficulty and uncertainty by observers carefully instructed and serving throughout a long period of practised observation. It appears, however, from a very large mass of such observations that wave-trains of like rate and energy, when introduced separately into the two ears with difference of phase, give rise to a tone whose *brightness decreases* as the phase-difference rises from zero and approaches one-half a wave length and as the localization of the tone migrates right or left from the median plane of the head. This change in brightness may be related to a central deformation of neural impulses which represents two phases of a wave-train at a single rate. No regular and consistent change in pitch, intensity, or volume was observed under these experimental conditions.

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AN EXPERIMENTAL STUDY OF VISUAL AND AUDITORY 'THICKNESS'[1]

By Emeline R. Moul, Cornell University

Historical setting. Experiments upon visual depth or 'thickness' have centered upon the control of physical and physiological conditions under which the perception is aroused. Hering recognized the psychological aspects of the problem, noting that under identical physical conditions we may see now a plane and again a distance.[2] Katz carried on the work begun by Hering, providing an account of the depth-like modes of phenomenal colors.[3] After Katz, interest in conditions was somewhat obscured by Schumann's alleged discovery that depth was a sensory phenomenon, which Schumann called the 'glassy sensation.'[4] In more recent studies, investigation of the influence of such factors as size, brightness, form, and fixation has kept an even pace with a parallel interest in description.[5]

Auditory depth, magnitude, or volume has a long controversial history and continues to occupy a prominent place in theoretical and experimental contexts. Discussion centers around two issues, the first its attributive status and the second its neurological substrate. Stumpf posited extension or volume (*Ausdehnung*) as an attribute of tones, observing that low tones are intrinsically massive while high ones are small.[6] Because it seemed to be inherent in the tone itself, he recognized volume as an attribute to be added to pitch and intensity.

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[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Bentley.

[2]E. Hering, in Hermann's *Handbuch der Physiologie*, Bd. III, Teil 1, 1879, 573.

[3]D. Katz, Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung, *Zsch. f. Psychol., Ergbd.* 7, 1911, 1-31.

[4]F. Schumann, Die Repräsentation des leeren Raumes im Bewusstsein, *Zsch. f. Psychol.*, 85, 1920, 224. Cf. also E. F. Möller, this Journal, 36, 1925, 249-285.

[5]H. Henning, Ein neuartige Tiefeneindruck, *Zsch. f. Psychol.*, 92, 1923, 161 f.; H. Schole, Beiträge zur Psychologie der Raumwahrnehmung und räumlichen Vorstellung, *Zsch. f. Psychol.*, 107, 1928, 314-365.

[6]C. Stumpf, *Tonpsychologie*, II, 1890, 56-60.

Like Stumpf, James and others (McDougall, Titchener, Watt, Köhler and Ogden)[7] make volume attributive. James applies 'voluminousness' or 'vastness' to experience of all modalities.[8] Rich has attempted by psychophysical methods to determine whether pitch and volume are co-variable.[9] His results, though meager, seem to indicate that the DL for tonal volume is different from that for pitch and that the two follow different courses. He concludes that they are independent attributes. Observational reports confirm the results of Rich's psychophysical methods. With practice his observers made volumic judgments as readily as reports of pitch. Rich repeated his work on volume under more adequately controlled conditions and verified his earlier results.[10] Halverson, again using a psychophysical method and finding the DL for volume larger than that for intensity, concluded that these are separate attributes.[11] Gundlach, however, attempting to equate the volume of pairs of unlike frequencies, is led to question its attributive status.[12]

In a recent discussion of auditory theory, Boring,[13] who regards tonal volume as a pre-spatial attribute, suggests as a neurological correlate the dispersion of neural impulses in the cortex, dispersion presumably varying with the number of basilar fibers stimulated. Troland further describes a hypothetical mechanism by which neural impulses from the basilar fibers are conducted to the cortex.[14]

Although there has been much research into the outside and the organic conditions of volume, the volumic aspects of visual and auditory experiences themselves have not been adequately described and compared. In the present investigation we have sought to discover by direct means the nature of such experiences in the two modes and to look for significant likenesses and differences be-

[7]G. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 126.

[8]W. James, *The Principles of Psychology*, II, 1890, 134 f.

[9]G. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13 ff.

[10]G. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 127-164.

[11]H. Halverson, Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360 ff.

[12]R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 195.

[13]E. G. Boring, Auditory theory with special reference to intensity, volume and localization, this JOURNAL, 37, 1926, 157-188.

[14]L. Troland, The psychophysiology of auditory qualities, *J. Gen. Psychol.*, 2, 1929, 28-58.

tween visual and auditory perceptions of this sort. In order to rid our observations of trite terms and rigid concepts, we have provisionally used the word 'thickness' in place of 'depth' or 'volume.'

The Experiments

General method. The visual and auditory stimuli chosen were of such a nature as to call out to a marked degree the characteristic sought. Therefore it was reasonable to expect that, under a general instruction, observers would give accounts of it in their reports. The *O*s were instructed to give a complete phenomenological description of their experiences, without information upon the purpose of the experiment. From the reports under general instruction, terms were selected that were most representative of the experience of 'thickness.' The next step was to present the same stimuli under more specific instructions. *O* was then directed to note and to describe that aspect which he had called depth, density, or thickness. Finally, in an effort to obtain a solution to the second part of our problem, an attempt at direct comparison of thickness in sight and hearing was made by giving a visual and an auditory stimulus in immediate succession and asking for reports upon likenesses or differences in this aspect.

Apparatus and procedure. Because of its pre-dimensionality and its comparative freedom from object-meaning, film-color was introduced in an early series, utilizing Martin's apparatus.[15] Illumination was furnished by two Corning daylight lamps placed slightly above and facing the color on the back screen. The intensity of the light could be varied and controlled by means of a rheostat and a voltmeter placed in the circuit. All stimuli were presented in a dark room. The *O*s looked through a lightless black hood at the outer gray screen where the film-color appeared.[16] The energy of illumination was represented by 70 or 110 volts and the exposure-time was 3 or 10 sec. As the experiment progressed, it became apparent from the reports that there were secondary cues to thickness in the film-color; the blackness of the hood through which *O* looked could be differentiated from the grey screen which bounded the film-color and the film color appeared on still another plane. In a later series we thereore undertook to provide a visual experience like film but one which would present a uniform field. Visual conditions were so arranged that *O* was

[15] M. Martin, Film, surface, and bulky colors and their intermediates, this Journal, 33, 1922, 459.

[16] The observers were Mr. D. T. Griffin (*G*), assistant in psychology; Miss M. Davidson (*D*); Mr. O. D. Anderson (*A*), and Mr. W. T. James (*J*), graduate students. All had at least three years experience in psychological observation. With the exception of *D*, all *O*s served throughout the experiment. The following Hering papers were used; gray 16, yellow 42, blue 89, blue 86, purple 14, green 9, green 66, red 19, orange 3, red 61, blue 12, green 74, red 1, and yellow 5.

confronted with a nearly homogeneous retinal field. Four inches before the eyes was a vertical plate of mud-ground glass 76 cm. wide by 61 cm. high. At a distance 20 cm. behind it another similar glass plate was set up. The plates were held upright by a grooved wooden frame. At 41 cm. behind the second a colored-glass sheet of the same size was placed in a frame. It was 1/8-in. thick and body-colored. Behind the colored glass, at 35 cm., was a cone-shaped galvanized tin reflector painted white inside and with its flaring base facing the colored glass. The reflector was 25 cm. deep and 75 cm. in diameter. Two daylight lamps of 200-w. each were focussed upon the center of it. The light-intensity was varied by a rheostat. A large black curtain closed off the view of the front screen and served to keep *O* from tactual cues as to the nearness of the front glass plate. After coming blindfold into the dark room, *O* was informally instructed to place his head in the headrest, to keep his eyes closed except when instructed to open them, to avoid exploratory movements with his hands, and to maintain a staring fixation straight ahead. He was given at least one minute for partial dark adaptation. Upon the presentation of the stimulus he looked into a visual field which was approximately uniform except for the extreme boundaries at the periphery. The light energies, expressed in volts, were 30, 60 and 95. It had been empirically determined with persons not observing in the experiment that the subjective differences of intensity as between these three energies were roughly the same. Colors were also presented whose intensity of illumination was gradually varied from 30 to 95 and from 95 to 30 volts within a single exposure.

For auditory stimuli, sounds were produced by an audio-oscillator and by tuning forks. The oscillator, which produces a practically pure tone of 1000 d.v. at three variable intensities, is Type 213, manufactured by the General Radio Company, Cambridge, Mass.[17] It was driven by four dry cells of 1½ volts each in a sound-proof box in a distant room and connected with a telephone receiver in the experimental room. The receiver was placed behind and slightly above *O*'s head and the sound set at a known intensity. Later in the experiment, in order to secure a more diffused and less localizable tone, which should be more obviously comparable to the diffused visual experience just described, the receiver was placed 5 ft. before *O*. A large phonograph horn above the receiver diffused the sound.

It was thought possible that tuning forks of various frequencies might yield a greater variety of descriptions and characterizations of auditory thickness. Forks of low frequency, *e.g.*, might result in a different sort of thickness, either in kind or amount, from the constant frequency of the oscillator. Forks of 700, 600, 400, 420, and 256 d.v. were therefore struck by a falling pendulum. A modification of Volkmann's sound-pendulum controlled the intensity.[18] The angle of fall (degrees of arc) was 10, 20 and 30. Again, five Max Kohl forks with properly tuned König resonators produced a sound which gave no localizing

[17]For a description see H. Halverson, The audio-oscillator, this JOURNAL, 38, 1927, 295.

[18]See E. B. Titchener, *Experimental Psychology, Instructor's Manual, Quantitative*, 1905, 195.

cue from manual excitation, since the tone was inaudible until the fork was placed before the resonator. The energy here was uncontrolled. The forks were 128, 192, 256, 288, and 384 d.v.[19]

A short series of noises completed the auditory stimuli in an attempt to determine whether noises afford any additional characterizations of thickness not reported with tones. The Volkmann pendulum was released and the hard rubber end struck against various substances, producing a brief sudden noise. For the production of continuous noises the surface of a rotating drum covered with sandpaper was scraped against a sheet of tin or glass. The speed of the drum was regulated by a system of pulleys and weights. All apparatus and the manipulations of *E* were concealed from *O*.

Throughout Part I (general instructions) the exposure-time was varied with a view to ascertaining whether such a variation had any effect on the volumic aspect of the experience. When no such effect was evident, the variations were discontinued and all stimuli were presented for 10 sec.

The formal instructions in Part I with both visual and auditory stimulation were:

"Observe attentively the exposed area (or the sound experience). After the experience disappears, report. The report should include a complete phenomenological description."

Since the purpose of the study was to obtain all possible characterizations and descriptions of thickness, it seemed best to have *O* report in any terms that were adequate to the experience. For this reason he was instructed to give a 'phenomenological' description. To be certain that the *O*s understood that the term meant simply to take experience as it comes, each *O* was asked, at the end of Part I, for a brief statement of what he had taken 'phenomenological description' to mean. The quotations show that the *O*s understood the instructions in the sense in which they had been intended.

A: "I consider a phenomenological description to be a description in meaningful terms of experience as it appears, as it is given."

D: "I understand a phenomenological description to mean a description of the appearance as a phenomenon, or a directly observable appearance. It is the description of the object or the thing in itself as it appears to the mind."

G: "When I am instructed to give a phenomenological description, I take it that my description need not necessarily be limited to dimensional or attributive terms. I try to tell how an experience is patterned and how it varies as it goes on, using any convenient terms which seem to indicate likenesses, differences, or variations."

J: "The sense in which I have used 'phenomenological' in the reports is to deal with and take experience in its own right, noting the most striking features, describing these in such terms as I can, without going back necessarily to attributes."

The stimuli in Part II (specific instructions) were the same as in Part I. *O* was formally instructed as follows:

"Observe and describe that aspect of the experience which you have called thickness or thinness, compactness, depth, volume, density, and the like. Also report temporal or spatial changes in the course of the experience."

[19]In *Quelques expériences d'acoustique*, 1882, 10-11, R. König gives the vibration-rates of the resonators.

In Part III visual and auditory phenomena were presented in immediate succession and directly compared. The colored glass was used for visual stimulation and the oscillator for auditory. Each of the three intensities of the sound was combined with each of the three intensities of the colors. The time order was so governed that neither invariably came first. Each visual and each auditory stimulus was given for 10 sec. The instructions read:

"You will be presented with a visual experience followed immediately by an auditory, or with an auditory followed by a visual. Observe and report any similarities or differences between the visual and auditory experiences with respect to thickness."

From the reports in Part II it was apparent that the *O*s had been taking thickness in two ways. It was, therefore, deemed advisable to make specific the sense in which they were to compare the visual and the auditory experiences. We first added to the instruction the qualification "thickness in the sense of size, voluminousness, spread in all directions." Then the whole series was repeated under the instruction "thickness in the sense of compactness, density, degree of concentration."

Results

(*1*) *Existence of a multi-directional thickness.* Experience sometimes possesses a characteristic of thickness when directly apprehended under the most general instructions. It appears under visual and under auditory stimulation and is referred to as tridimensional or as extending in several or in all directions. Similar and identical terms are used to describe the visual and the auditory experiences; and they appear in both noises and tones. Under our conditions of stimulation and instruction, thickness is never definitely bounded. It comes as a simple and immediate aspect of the experience. Its size is frequently relative to some previous experience, but only when *O* attempts to report the 'amount' of it. The following representative reports obtained under general instructions show the presence of the characteristic of depth, size, spread, or volume.[20]

Film-color.

A: [orange-110v.] Phenomenologically it is deep and goes back of the form which contains it. The distance or thickness I cannot say, simply because it isn't that sort of deepness, not a rigidly contoured, perfectly definite extension out there; but extended as a mist is extended.

D: [green-110v.] The impression was very strong that the mass of color extended indefinitely beyond the aperture, *i.e.* up and down and on the sides.

[20]The first letter refers to the observer; next is the quality of the stimulus; next the intensity (for the colors, in voltage of the illuminating circuit; for the tones, either the force of the oscillator-setting—high, medium, low—or the force with which the tuning fork was actuated—loud, medium, soft). Where no exposure-time is given, the time was 10 sec.

G: [yellow-100v.] The fulness or richness or thickness is as if I were looking into more or less dense fluids of the color of the disk. No front or back bounding plane.

J: [orange-110v.] It had considerable spread in every direction.

Tones.

A: [1000 d.v.-high: 3 sec.] This is more space-filling, *i.e.* it extends out there in several directions, up and down. The endings of up-down, back-forward, left-right, are extremely indefinite.

G: [1000 d.v.-high] This was more limited in extent and more definitely bounded.

J: [400 d.v.-loud] It was spread out. It did not strike one as being in a small stream, but as radiating everywhere. The bigness of the thing was one of its most outstanding characteristics.

Colored glass.

A: [yellow-95v.] There is no cue to specific thickness or distance. There is no such thing as direction in the depth at all. It seems to go out and back in all directions. It just isn't deep on a line straight out from my eyes. There is never any hint in any of these that I could take a yardstick and say there's where it ought to stop; and yet somehow the thing begins right out before my eyes.

D: [red-60v.] It gave a very pronounced idea of great mass. It was as though a thick red cloud cut off this part of the world from all the rest.

G: [green-95v.] This had as its most outstanding character that I could keep looking into it. There was no definite stopping point to my gaze. It had considerable volume.

Noise.[21]

A: [7] It isn't soft in the sense that the tones are soft. They have an almost glowlike spread in three dimensions.

D: [6, 7, 8, 9] All these four sounds seem localized a few feet on the other side of the curtain. They did not give the impression of indefinable distance nor of diffuseness which the tones gave.

G: [1] Although it did not have a specific volume of its own, it was voluminous.

(*2*) *Description of the experience of thickness.* The psychological description of thickness is not an easy task. Under general instructions *O*s gave detailed accounts of widely different aspects of their experience. They reported on the quality, duration, and texture of colors and sounds, the beauty of the colors or the roughness of the noises as well as on thickness. In finding expressions that adequately characterize the experience, the *O*s frequently resort to some context, simile, or meaning upon which the perception appears to depend. These settings are so varied that they are almost indeterminate. The characterizations, however, do fall roughly into three groups.

First, there are the descriptions in which there is little or no dependence upon contexts, as when the experience is described as 'concave,' 'soft,' or 'penetrable.' These are comparatively simple

[21]The noise stimuli and their numbers were: 1 wood, 2 corrugated paper, 3 felt, 4 rubber, 5 leather, 6 tin (one weight), 7 glass (one weight), 8 tin (three weights), 9 glass (three weights).

descriptions in the sense that the characterization fits the experience directly, without recourse to a more complex situation. We shall call this type *unsupplemented descriptions.*

Secondly, the perception may be supplemented by certain contexts, *e.g.* it may be described as bound-up-with, and dependent-upon, a particular grained aspect of a color. In this type of context thickness and the contextual material are of the same modality, both visual. In apprehending the organism uses the context presented by visual stimulation. This type of descriptions will be referred to as *intra-modal supplementary contexts.*

In the third group, the organism uses resources from other modalities, *e.g.* a tone is apprehended as 'thick' or 'thin' on the basis of the size of an accompanying visual image. These we shall call *extra-modal supplementary contexts.*

Group 1. Unsupplemented descriptions. Experiences possessing depth, volume, or thickness are described as soft, yielding, penetrable, compact, smooth, or globular. These characters are co-existent with, and somehow imply or carry the meaning of, thickness. It is significant that the list of unsupplemented descriptions just given appears both in vision and in audition. The inference is that the organism perceives thickness in the same way under various modes of stimulation; and that visual depth and auditory volume are not totally different perceptive moments. It will be remembered that these descriptions were obtained in separate series and under general instructions. The observers were working without knowledge of the problem. No suggestion as regards a comparison was given until the last series, in which *O* was asked directly to observe similarities and differences.

Film-color.

A: [yellow-70v.] The color experience is somehow inherently soft and penetrable. Its penetrable nature, I think, constitutes its depth.

D: [orange-110v.] It seemed to possess somewhat greater depth or density than the preceding; although this depth had no definite limits.

G: [blue-110v.] The disk was somewhat 'look-into-able.' The blue was rich, deep and penetrable.

J: [blue-70v.] It has a sort of substantiality about it. It doesn't quite fit to call it thick; but I suppose that is what I mean, a substantial consistency with a body to it.

Tones.

A: [700 d.v.-soft] It was extremely smooth, rather mellow, penetrable. I wish I knew what penetrability means in this instance. It was soft. Softness seems to be penetrability. The sound somehow carries the meaning of 'giveyness', of penetrability, of a certain looseness, a certain softness in the

sense that a thick vapor is soft. This softness, this penetrability, is very closely bound up with the meaning of spread, of tridimensional extensity. It means on its very face that it goes in all directions.

G: [1000 d.v.-medium] It was thick, dense, compact.

A: [600 d.v.-medium] Qualitatively the tone is mellow, soft, yielding and smooth-textured, like a film-color. I get the same sort of experience from a film color; —an indefinite thickness, a smooth texture, a lack of hard and fast borders. The only difference is that one is visual and one auditory.

Colored glass.

A: [red-30v.] The depth is undoubtedly a filmy depth, filmy in the sense that it is not definitely localized as to position away from me and in the sense of its being soft and penetrable. Its extent is extremely indefinite. All that I know is that the total thing is a globular mass, fairly luminous.

D: [yellow-30v.] Its most prominent characteristic was its thickness and opaqueness. There was no feeling that one could have seen through it.

G: [lavender-95v.] Everything was pinkness. It was penetrable as if the pinkness were a substance in which I was immersed.

J: [lavender-95v.] It was more like a film than anything else; it had no beginning or end. I seemed to be engulfed within it.

Group 2. Intra-modal supplementary contexts. Our stimuli had been carefully chosen so that secondary cues to depth and volume would be eliminated as far as possible. But under the difficult task of observation the *O*s sometimes apprehended the thickness aspect in terms of other constituents which they found in the visual or auditory content. Such constituents are color, pitch, and intensity. These contexts and the particular way they appear may be grouped under three heads.

Quality. The color or the pitch bears a relation to thickness.

Intensity. The degree of brightness of illumination or the degree of loudness may furnish a cue. Quality and intensity frequently overlap. The *O*s are uncertain about the possibility of separating them, particularly in audition. In an experiment on tonal volume, Rich at first found similar inability on the part of his *O*s to be certain what volume was.[22] After a period of training, however, judgments of volume were made as readily as those of other attributes. Our results in no way conflict with Rich's. His problem was one of analysis. We have attempted to describe volume as it comes, unstripped of experiential accompaniments. Whether the description can be made in terms of simples or whether it is sometimes bound up with other aspects, the results are equally valuable for our purpose.

Texture. Textural variations, such as the granular appearance of a color or the 'wiry' aspect of a tone, are reported as determining factors.

[22] G. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13 ff.

In the quotations below the overlapping of the three types of contexts is evident as well as the similarity between vision and audition.

Quality.

A: [red-30v.] That's a very thick, dense, impenetrable thing. I feel that the color and the degree of illumination have a great deal to do with this.

G: [blue-95v, decreased to 30v.] It was less thick at the end when the blueness dropped out.

A: [1000 d.v.-high: 3 sec.] That pitch carries with it the meaning that it is not bulky, nor is it extremely fine or thin. The spatial meaning that is referred to by the loudness is that the room is filled up with it. The two uses of the spatialness of sound—the space of the pitch and the space of the loudness—are very confusing. I can't observe my way through them. I can't take pitch and volume separately.

G: [256 d.v.-3 sec.] It has the same mass as a pure tone of very low pitch

Intensity.

A: [yellow-60v.] There are on this illuminated area, near the center, spots of a diffused nature. All that I can say about them is that they are simply darknesses in this light area. The nearer the center of this area, the deeper the phenomenon is. Those darkish areas are nearer to me than the smooth part of the colored area.

D: [red-30v.] When the experience first came, the red appeared thinner than it did later when it became darker.

G: [yellow-60v.] It seemed as if there were a number of bright points in it. Where these points were, *i.e.* between the darker spots, light was coming through somewhere.

A: [1000 d.v.-low-6 sec.] The loudness has a great deal to do with how big the thing is, *i.e.* how much space it takes up. The fact that it was less loud physically determined its being taken as smaller. Further, the pitch is such-and-such an extent. There is an intimate relation between the pitch and the volume so far as its depth or thickness is concerned. I can't take these things separately.

G: [1000 d.v.-low] I am rather bothered about volume. I feel uncertain of my volumic judgments. It was less loud and that determined its being taken as more limited, smaller.

Texture.

A: [red-110v.] Fine particles make up the thing. The particles are alive; they don't stay put. They are working and interweaving among themselves. They are almost sparkle-like. I feel that the depth is due to that peculiar liveness of the graining.

G: [purple-110v.] It had over it a certain lability. It was as if I were looking over a field of such color which happened to be over a heated surface. Corresponding to this there was an increase in the depth-volume.

J: [purple-70v.] It was a little rough. There was something like small particles in it that made it seem diffused.

A: [320-soft] There is a rapid sifting down of extremely fine particles. It is alive, moving, progressing. This sort of onward flux is no more enclosed by specific edges than is a mist. It is there and it is thick.

G: [1000 d.v.-high] Here I think that thin means tiny, pinched up together.

J: [1000 d.v.-high] It is very thin and very compact.

Group 3. Extra-modal supplementary contexts. Thickness may be realized through an imaginal context, *i.e.* one of an unstimulated modality. In vision these contexts were chiefly tactual; in audition, predominantly visual, rarely kinaesthetic and tactual.

There is, in Group 3, greater variation among the *O*s in the degree to which they relate thickness to contexts. Thus we are led to believe that such imaginal supplements are not a fundamental part of the perception, but individual cues to it. Reports in Group 1 have demonstrated that the perception is possible without additional aids. In more than 150 reports, *G* refers to them but once; with *A* and *J* they appear more frequently; while *D* uses them in more than half the reports in the auditory series. Our study was not quantitative and therefore an attempt to quantify any aspect of our descriptive reports will be exceedingly rough; but the table below shows the trend of the type of contexts used and demonstrates the wide individual variation.

EXTRA-MODAL SUPPLEMENTARY CONTEXTS

Stimulus	*Context*	*Obs.*	*Reports mentioning context*	*Total reports*
Visual	Tactual	*A*	6	67
"	"	*D*	6	47
Auditory	Visual	*A*	8	62
"	"	*D*	30	53
"	"	*G*	1	89
"	"	*J*	20	59
Tactual		*A*	7	62
"		*J*	2	59
Kinaesthetic		*A*	1	62

For *D*, the imaginal forms assumed various shapes and designs. So insistent were these images, that, when *D* attempted to separate the visual image from the sound, the sound was again represented by another visual image.

D: [1009 d.v.-medium] There seemed to be two characters which could be attended to. If I attended to the visual image, it remained thin and narrow; but if I attended to the sound itself, it was very diffuse. Even though I was considering the sound in contrast to the first visual image, the sound was translated also into another visual image.

J consistently reported visual images of two types: for small thin sounds, a streamlike image; and diffused sounds were represented by a circular, wavy visual form. The *O*s were uncertain about the exact nature of these contexts. They were called images, meanings, or the experience "was represented by" the formations. It is probable that all these extra-modal supplements refer to some sensimaginal resource of the organism to perceive more completely the object presented. The following reports show the varieties of imagery and the relation between these contexts and thickness.

A: [blue-110v.] I feel that I could have poked something through that circle and my stick would never have been long enough to reach the bottom of the thing. [128 d.v.] This sort of extensity is put in visual terms. It appears as spread-out, grayish, massive, loose stuff that is in motion.

D: [red-60v.] There was a strong conviction that one could immerse one's hand in it. [1000 d.v.-medium] The visual image that presented itself is of a straight line running horizontally about 1/8-in. in thickness and below that an area about twice that size in a lighter gray.

G: [green-30v. increased to 95v.] It seems possible to sink one's finger into this greenish light field, although the voluminousness is not unlimited.

J: [256 d.v.-3 sec.] It was visualized as a stream of water flowing around in every direction from a point. Waves travel in every direction. [1000 d.v.-high] It was like water running over my hand.

(*3*) *Descriptions under specific instructions.* When *O*s were definitely directed to describe the depth, volume or thickness which they had referred to in previous series, they had difficulty in making a clean-cut separation between two meanings of the term. Under general instruction there had been hints of this confusion, but it never became explicit until the later series. These meanings were (1) of spread, size, amount of space taken up, and (2) of compactness or condensation. Though distinct, these two meanings are not unrelated. When an experience is 'very outspread' it is usually also referred to as possessing a low degree of condensation, loosely woven. Smaller experiences are hard, more compact. This confusion of terms might have been avoided somewhat if we had defined for *O*s what we meant by thickness. But that was precisely what we wanted the *O*s to characterize. We were unwilling to separate the two meanings or to suggest, by the instructions, any relationship between them. We preferred to have the *O*s, during the course of observation, come upon whatever differences of relation were there. Fundamentally the difference seems to involve the aspect of the pattern which *O* notes. If he regards the outlines, the contours, he reports thickness in the sense of spread; if he regards the pattern's inner structure, the way it is textured within its borders, it is taken in the sense of compactness.

(*4*) *Incidental results.* (*a*) *Relation between strength of stimulation and localization.* In the visual series, at least, there was an apparent relation between the strength of the stimulus and the definiteness of localization. Experiences from less intense illumination were found to be more localizable than those of greater intensity. This is possibly due to the presence of contours, for when illumination is low there is a slight suggestion of contour which serves as a

cue for vague localization. With medium or intense illumination, there are no borders and no localization. We do not feel justified in inferring any *direct* relation between the intensity of the stimulus and the definiteness of localization, therefore, without further investigation in which the factor of contour is controlled.

Rigid control of auditory intensity was possible only with the sounds from the oscillator. Reports of differences in localization are inconsistent and therefore inconclusive.

(*b*) *Development of thickness during temporal course.* Temporal variations from small to large spread, from surface to depth, from localizability to non-localizability, and from object-meaning to non-objectivity occurred throughout the reports under general instructions. They occurred with visual and with auditory stimulation. At first sight it might appear that there are successive stages in the development of the apprehension of thickness. But when we turn to the reports given under specific instructions, even though the *O*s were directed particularly to report upon thickness and any temporal or spatial change in the course of the experience, we find no such development. *A* and *G* omit all reference to temporal changes, and *D* and *J* state positively that there were none. This is somewhat difficult to understand and to interpret. Had such developments appeared only under film-color conditions the solution might have been sought in accommodation. *O* accommodated first for the gray screen and later for the color beyond the opening. We find, however, a like occurrence in the colored-glass series, where changes in accommodation are improbable. We have, too, a similar variation with auditory stimulation, both before and after initial localizing cues had been eliminated. Our stimuli were not varied qualitatively throughout their course.[23] Since the phenomenon appears under both ocular and aural stimulation, the explanation lies without the physical or physiological conditions of the experiment.

It is not probable that such a temporal development is representative of the way thickness appears in experience for, when the same outside conditions were operative but the instructions different (specific), we find no such change. The factor respon-

[23]The changes in experience under variations of stimulus (when the intensity of the illuminating circuit was gradually increased or decreased within a given exposure) do not belong here and will be discussed in the following section.

sible for this variation, then, must be looked for in central conditions. We offer an explanation in terms of a variation in attitude. Under general instructions *O* was predisposed to look for many aspects of the experience. When the stimulus appeared, he ran the gamut of characterizations that were possible with that particular type of stimulus. In shifting from a predisposition to note color to one of noting form, etc., the function may have been accomplished more slowly than when the predisposition is definitely in one direction. During this shift the experience of thickness is taking place gradually. Disposed as he is, *O* notes only the shift in his own performance and not the attitudinal change. But when he has been instructed specifically for thickness, it is apprehended immediately upon its appearance and there is no temporal shift.

In the series of noises which were presented for a very brief interval, no such changes were reported. If, as we maintain, the variations in thickness depend upon shifts in attitude, it is conceivable that in the 'noise' series the organism was prevented from making attitudinal adjustments on account of the brief duration of the stimulus.

The following reports, given under general instructions, illustrate the phenomenon.

A: [green-30v.] I've never seen surface gradually become film before. First there is a surface, located quite definitely. Suddenly objectivity is taken off it. It softens up, becomes more diffuse, more alive. Gradually there is no surface, no localization. I can't say anything about its depth except that I could look back into it and that it was possibly closer to me than the surface. There was no hint as to how far or how close it was.

J: [blue-30v.] I could literally see it spreading out towards me. The most outstanding thing was the growing of experience. After it reached the highest point of spread it seemed to be right at my eyes.

A: [1000 d.v.-high] It seems to grow and expand in all directions. I don't believe the growing is visual or tactual. The sound itself is simple *spread.* It wells up. It is a spread of penetrable material.

D: [1000 d.v.-low] The volume did not remain constant. It seemed to be rather small; but it gradually became more voluminous. It seemed to widen, spread out. Its breadth became greater. It became more smooth and even. During the latter part, the tone tended to fill experience. It ceased to be associated with any object. At first the sound was there on the other side of the curtain; but later this sense of distance completely vanished.

G: [128 d.v.] The temporal course might be divided into two parts, the second of which closely resembled the first. The second lost the 'meaningfulness' of the first. It was less 'bodiful,' less dense.

J: [600 d.v.] After listening to it, it lost the 'there' reference and became non-localizable. It spread out everywhere.

(*c*) *Emergence of filmy thickness from a 'surfacy' experience, with gradual increase of illumination.* When the illumination was

gradually increased during a 10-sec. exposure, experience became more and more penetrable and filmy and when decreased less. No new qualitative descriptions resulted. Reports of the transition are given below.

A: [lavender-30v. increased to 95v.] At the beginning the thing is almost surfacy about 6 inches away from my eyes, a ragged surface. As it went on, it gradually became softer. Those sharp irregular daubs dropped off. Then a true film appeared, the localization of which was extremely indefinite.

G: [yellow-30v. increased to 95v.] Its filmy plane-likeness seemed to lose whatever localization and surface character of that sort it had had.

J: [red-95v. decreased to 30v.] It extended everywhere before me. It began to back off into the background and to become more dense. At this point it takes on an almost shiny character. At this stage a surface meaning comes in. It didn't have the film character at the end that it had at the beginning.

(*d*) *Differences in thickness between noises and tones.* Our purpose in introducing a series of noises was twofold; it provided a greater range of stimulus and it might reveal significant differences between noises and tones. The reports are probably in large measure a function of our choice of stimuli. The noises were of shorter duration than the tones. We chose noises that were qualitatively simple and which would be perceptualized as little as possible. Therefore, in the interpretation of results, allowance must be made for this temporal difference. The noises were compact, small in extent, dense and impenetrable.

A: [1] The thing was so concentrated that it took up only a little space. It was tight, hard and impenetrable.

D: [1] Its volume was very small. I believe this was influenced by the brevity of the experience and, partly, by the small visual image.

G: [2] It was very small, impenetrable, compact.

J: [3] Was a dull, woody-like thing. It also had spread. It was more easily localized than those that continue for a time.

We have here nothing qualitatively new. The characterizations read quite like those of tones. Localization occurs as with tones before the cues were eliminated. We conclude, therefore, that on the side of volume or thickness noises are much like tones.

(*5*) *Direct comparison of thickness in vision and audition.* When the *O*s were asked to note any similarities or differences between the visual and the auditory experiences in respect to thickness, it was found that there were no gross differences; the differences were always of minor degrees.

A: [1000 d.v.-low, followed by red 30v.] The sound is very small, weak, little. The visual experience was somehow of the same order of size. They were very similar in that their extent in all directions is indefinite and rather soft.

D: [red-95v., followed by 1000 d.v.-high] Although the sound as it continued tended to be spread out in space somewhat the way the color was, it never occupied as great a space.

G: [blue-95v., followed by 1000 d.v.-high] The sound is very much smaller than the visual experience. The latter had a considerable expanse and volume. The former was relatively tiny.

J: [blue-60v., followed by 1000 d.v.-low] The spread and volume of the light were in every direction. The sound was the same way.

In comparing the 'extent' of a visual with an auditory experience, the method of comparison or the criterion of judging degrees of thickness was not precisely uniform for all the *O*s. They do not state specifically *how* they compare; they simply give the results of the comparison. (It will be recalled that our instructions merely directed them to report likenesses or differences; not to comment on the process of comparing.) Obviously our only source of knowing what they did is from the comparisons reported. We can detect two main types of performance. *A* and *J* place a given auditory experience on a scale of auditory extents and the visual on a continuum of visual extents. The relative positions on the two scales are compared. *D* and *G* placed all experiences on a single line, so to say, with auditory at the small and visual at the large end. In reports on density, *A* and *J* again judge in terms of separate continua and compare colors and sounds on the basis of relative position on the continua. *D* and *G* place all the visual phenomena on one end of the scale.

The significant thing, however, is not these slight differences in the mode of performance; it lies rather in the agreement of all the *O*s that there is in visual and in auditory experience a thickness aspect that is directly comparable. We are dealing not with unrelated perceptions of visual depth and auditory volume, but with an aspect of experience which is fundamentally the same whether mediated by the eye or by the ear.

Conclusions

(1) There is a pre-spatial attribute of thickness in vision and audition which is clear-cut in observation. The list of descriptive terms for this 'pre-perception' is practically identical for the two modalities. In both, certain types of experience are characterized as 'big,' 'soft,' 'penetrable,' 'diffuse,' 'filmy,' 'dull,' and 'vaguely contoured'; while others are referred to as 'small,' 'hard,' 'opaque,' 'compact,' 'surfacy,' 'bright,' and 'rather well defined.' The immediate descriptions *may* be supplemented by other contexts,

either from the modality stimulated or in sensimaginal terms. From the wide individual variation in the nature and frequency of these supplementary constituents, we conclude that they are not essential to the apprehension of thickness. The 'pre-perception' is simple and possesses a common character in vision and audition. By simple we mean that, when directly observed by the method of inspection, it is not further analyzable.

(2) Thickness in sight and hearing is not only characterized in identical terms, but is comparable first-hand. Observers without confusion or hesitancy describe and comment upon the thickness of visual and auditory phenomena occurring in immediate sequence. The two modes of stimulation are taken as a single presentation would be; a presentation whose successive aspects are noted and reported as similar.

THE RANGE AND MODIFIABILITY OF CONSONANCE IN CERTAIN MUSICAL INTERVALS

By Joseph Peterson and F. W. Smith, George Peabody College

It is now well-known that simplicity of ratio does not in itself determine the degree of consonance of any interval. Aside from the fact that no mechanism is known by which the ear can react to such ratios directly, it is to be noted that most of the consonant intervals in the equal temperament scale are, as a matter of fact, of very complex ratios, while others, not tolerated at all as consonances, are far more simple. Moreover, it can no longer be maintained that reaction to consonance is purely a cognitive affair, as writers like Stumpf maintained on the basis of an older psychology. It is still good technique in experimental work to avoid the disturbances of affective and emotional factors as far as possible, but we cannot escape from effects of habituation from which no subject is free and which make interesting and constructive differences in the reactions of members of different ethnic groups.

It is also generally known that a number of physical factors and physiological conditions common to all reactors (*e.g.* clashing partial tones or difference tones and roughness between difference tones and fundamentals, as well as physiological conditions in the ear which bring about interference of all periodicities approximating one another in frequency) make certain interval ranges between pairs of gradually diverging tones intolerable; and it is also admitted that between certain of these intolerable ranges lie possibilities of consonant relationships which have not all been cultivated yet by our musical environment. A multiplicity of factors of different kinds unite in different combinations to produce various degrees of tolerance and of preference among the so-called consonances that have evolved in our own civilization. Certain of these consonant intervals are so definitely and favorably located by the physical and the physiological factors, as to be found in common use by practically all ethnic groups. Here we think especially of the octave, the fifth, and the fourth. But even

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these intervals, which have the simple ratios of 1:2, 1:3, and 3:4, respectively, receive about the same sort of reaction from both musical and non-musical subjects when they are slightly mistuned (*i.e.* when they diverge slightly from their true frequency relationships) as when they have the frequency ratios indicated. While this general fact is well known and is employed in the use of the equal temperament scale, we need more data on the reactions of both musically and non-musically trained subjects to the several consonant intervals as they are thrown out of their most simple, or 'true,' relationships in varying degrees. It is this problem to which we have devoted ourselves in the present investigation, which is to be regarded as only preliminary, even though some of our results are highly interesting and will probably be supported in further researches.

Our specific problem was to ascertain how far certain musical intervals in just temperament may be augmented or diminished without marked effect on the reactions of our subjects.

The Experiment

A set of Edelmann tuning forks of fine quality and exact pitch, together with variable forks of closely similar tone quality, were used, all forks being mounted on resonance boxes. The *S*s were 36 college students, half of whom had had musical training in various lines extending over a period of from 3 to 8 yr. The mean training was 5.11 yr. The other 18 *S*s had had no training in music whatever. The forks were arranged in pairs, each pair including the tonic (c', 256 d.v.), so that the experimenter, holding in the right hand two rubber mallets, the handles of which were bound together at a fixed angle, could actuate simultaneously any pair of forks which he desired to sound. The rubber mallets were covered with cloth so as to eliminate any high overtones which might disturb the *S*s, and the forks were sounded so gently and uniformly as to keep the intensity down just to the easily audible degree. Under these conditions the combination tones were not prominent enough to produce perceptible clashes or roughness, and the upper partials were eliminated for all practical purposes. The tones were therefore practically pure tones uncomplicated by secondary physical factors. Every interval sounded was permitted to continue for approximately 3 sec. In every trial *E* sounded these six intervals: major second, major third, perfect fourth, perfect fifth, major sixth, and major seventh, in the order here given. Every one of the *S*s recorded on prepared sheets the one of the four consonant intervals which seemed to him 'to be changed or unnatural.'

The experiments were carried out on individual *S*s, except for a few cases who were tested in groups of not over four. Every *S* sat with his back toward *E* about 4 ft. away from the sounding forks, under conditions which excluded visual cues. Preliminary exercises and instructions, which were uniform for

all *S*s, made clear what was to be done. Every *S* was instructed that in each trial one of the four consonances—*i.e.* one of the four intervals not at the extremes of the series—would be changed slightly, and he was asked to write down on prepared blanks after the trial the number of the consonant interval (3, 4, 5, or 6 for major third, fourth, fifth, and major sixth, respectively) in which he noticed a change, or which seemed to be 'unnatural.' In all trials in which no such changes were noticed he was permitted to omit any judgment, and was asked simply to make a check mark. This, of course, introduced an unequalizing factor into the responses in that some *S*s will be more likely than others to attempt no judgment on practically the same degree of uncertainty. But since many of our *S*s were not acquainted with psychological methods we wanted to avoid forcing upon them the seeming unnaturalness and restriction of having to give a judgment of difference when they perceived no real basis for it. To compensate for and throw light on this lack of uniformity of conditions in judgments by the several *S*s, we took the record of every *S* after the experiment was completed and marked every fourth omission on every interval correct, since this ratio of responses would have been correct by pure guess. In the tables giving results we treat both kinds of data; that is, judgments under conditions not requiring a guess when no change is noticed and 'judgments' handled as if a guess were required. It is to be understood that the data of the latter type have been modified as explained, since it would be impossible to get both kinds of judgments on the same situation. This second type of record, so far as we can observe, is practically what it would be if the *S* had been compelled to guess in all cases of uncertainty, except that the percentages right would probably be slightly larger for both groups.

The mistuning of each of the consonant intervals was effected simply by the substitution of the variable fork for the tonic without any shifting of the positions of these forks. The variable forks had previously been raised or lowered in pitch by a predetermined number of vibrations per second, according to a fixed program. Every one of the four consonant intervals was thus altered 9 times by these several amounts: 10, 8, 5, 3, 7, 6, 4, 2, and 1 d.v., and in the order given, and 10 judgments were secured in a random order on each modification. Half of these modifications were made by raising the pitch of the variable c'-forks and half by lowering it by the number of double vibrations indicated. The former process, of course, diminished the intervals, while the latter augmented them. In addition, 20 trials were given every *S* with no alterations in the justly tempered intervals, to make sure that he was not reacting to mere quality differences in the c'-forks. These 20 trials occurred in an irregular order among the other trials and the *S*s did not know in those cases that no interval was modified. Thus 380 judgments were obtained from every one of the 36 *S*s.

Results

In Table I the general results are given in the form of the percentage of correct judgments on each deviation both by the 18 musically trained (MS) and by the 18 non-musically trained (NMS) subjects. The percentages actually correct are given under

A, and the percentages which would have been right if the subjects had been forced to guess on all cases of uncertainty are given under B. The differences, in double vibrations (d.v.), in the percentages correct by the two groups are given in the column at the right under A and B, respectively; and at the extreme right of the table, under the caption B–A, are given the differences between B and A, which differences constitute, for each group of *S*s separately, the percentage of 'right judgments' that were added by the method employed in B (already explained) for each degree of divergence. It will be noted from the differences under A and B

TABLE I

PERCENTAGE OF CORRECT JUDGMENTS IN THE ENTIRE TEST BY 18 MUSICALLY TRAINED (MS) AND 18 NON-MUSICALLY TRAINED (NMS) SUBJECTS

Deviation of interval in d.v.	A Changed interval indicated if noticed MS	NMS	Diff.	B Guess is made when no change is noticed MS	NMS	Diff.	B–A MS	NMS
10	89	48	41	90	52	38	1	4
8	87	53	34	88	55	33	1	2
7	84	49	35	85	56	29	1	7
6	75	47	28	77	52	25	2	5
5	55	31	24	63	40	23	8	9
4	29	16	13	43	32	11	14	16
3	12	7	5	30	31	−1	18	24
2	3	3	0	27	27	0	24	24
1	2	1	1	26	27	−1	24	26
0	1	2	−1	26	26	0	25	24
					Totals		118	141

that the music *S*s far exceed the non-music in accuracy in all cases of divergence of over 3 or 4 d.v., but on smaller modification these differences do not occur. About this fact more will be said later. If we get the totals of the percentages given for each of the two groups of *S*s in the columns under B–A, and divide the NMS total by the MS total (141/118), we find that the non-musical omitted judgments on an average of 19% more trials than did the musical when both groups were left free to omit judgments about which there was uncertainty. Under the conditions of B, already described, approximately 25% of all judgments will be correct by the laws of chance. This percentage is approximated closely by both groups of *S*s when degrees of modification of intervals are so small (0 to 3 or 4 d.v.) as to be ineffective.

The subsequent tables give data only as if all students were compelled to judge in every trial, that is, to guess whenever there was uncertainty.

Table II compares the two groups of *Ss* as to percentage right judgments both on the augmented and on the diminished intervals. The important thing to note in this table is the very great con-

TABLE II

PERCENTAGE OF CORRECT JUDGMENTS IN THE ENTIRE TEST ON AUGMENTED AND ON DIMINISHED INTERVALS BY 18 MUSICALLY TRAINED (MS) AND 18 NON-MUSICALLY TRAINED (NMS) SUBJECTS

Deviation of interval in d.v.	Augmented intervals			Diminished intervals			Aug.–Dim.	
	MS	NMS	Diff.	MS	NMS	Diff.	MS	NMS
10	89	56	33	91	49	42	−2	7
8	86	61	25	89	49	40	−3	12
7	86	58	28	83	55	28	3	3
6	74	49	25	80	53	27	−6	−4
5	60	38	22	66	41	25	−6	−3
4	40	32	8	46	32	14	−6	0
3	26	31	−5	34	31	3	−8	0
2	26	26	0	27	27	0	−1	−1
1	25	28	−3	26	27	−1	−1	1

sistency with which the music students were more correct on the diminished than on the augmented intervals (shown by the negative signs under the column MS on the next to the right column of the table). This same consistency is not found in the results of the non-music students, as shown in the column at the extreme right. This difference in the two groups we shall attempt later to explain when we shall have considered other data.

Taking the just noticeable difference (j.n.d.) in divergence of each of the four intervals from its perfect ratio as being half way between the percentage right by pure guess (25%) and that yielded at the first point of perfect certainty (100%), *i.e.* $25+(100-25)/2=62.5$, we get for the several intervals the results for the MS group shown in Table III.

TABLE III

RANGE OF UNNOTICED CHANGE BY MUSIC STUDENTS

Interval	Range of unnoticed change in d.v.		Range of unnoticed change in 'cents'
	By the A-method	By the B-method	By the B-method
Fifth	4.45±0.64	4.70±0.60	29
Fourth	4.54±0.64	4.95±0.66	31
Major Third	4.95±0.68	5.15±0.70	32
Major Sixth	5.27±0.75	5.74±0.76	36

Comparable data for the NMS group are not available because even the median point (62.5% right by the B-method and 50% by the A-method) was not reached by this group of *S*s within the range of 1 to 10 d.v. used in the present experiment; *i.e.* the range of modification unnoticed by the non-musical students in the four intervals and at the pitch considered here was over 10 d.v. It is evident from the data in Table III that the range of unnoticeable divergence from the justly tempered intervals for music students is slightly greater in the case of the major third and the major sixth than in that of the fifth and the fourth, but to test the reliability of this difference it is necessary to use larger groups of *S*s than were employed in this experiment. Our unnoticeable difference of 29 'cents' for the fifth and of 35 'cents' for the major sixth is not reliable for the 18 *S*s.

TABLE IV

THE PERCENTAGE OF ERRORS MADE IN SUCCESSIVE TRIALS

	Trials									
*S*s	1	2	3	4	5	6	7	8	9	10
Musical*	12.1	11.9	12.2	12.3	10.3	7.1	7.9	9.2	9.1	8.0
Non-musical†	15.6	14.6	11.6	10.8	8.5	8.4	7.7	7.7	7.7	7.2

*324 judgments at every trial.
†270 judgments at every trial.

Our experiment incidentally yielded some direct data on practice effects of the judgments. Taking together all the errors made on the fifth, the fourth, and the major third (nothing different was evident on observation in the major sixth, so it was not considered), we have errors made in 30 judgments by every one of the 36 *S*s (1,080 in all) on each degree of divergence from 1 to 10 d.v. Since 10 judgments were obtained from every *S* on every degree of divergence on every interval, it is possible to classify the pooled results of all the *S*s into 10 consecutive trials and to find for every one of these ten trials its percentage of the total errors made in all the ten trials. We have worked out these results separately for the musical and the non-musical *S*s (except that the results do not include the judgments on intervals of less divergence than 4 d.v. for the MS group, and of less than 5 d.v. for the NMS group; because success within this range was due almost wholly to chance). These results are shown in Table IV. They indicate considerable improvement due to practice through the 10 suc-

cessive trials, despite the fact that the *S*s did not know the results of their judgments until the entire series of trials was completed. The improvement by the MS group from the first to the 10th trial is 36% and for the NMS group it is 54%. It is appreciably greater in the case of the non-musical than in that of the musical students.

Now, in Table V, we come to results which seem very important both from the standpoint of the psychology of consonance and dissonance and also from that of the possibilities of changes in intervals in actual musical practice. At the top of this table are given in separate heads the four intervals about which we have been concerned in this paper. Going down each of these columns, one finds first the ratios in both just and equal temperament and then the actual vibration numbers. The next line below this in each column indicates whether the interval in equal temperament is augmented or diminished as compared with its size in just temperament. Here we find that for the pitches used in this experiment the equally tempered interval is smaller than the justly tempered by 0.4 d.v. in the case of the fifth; that in the fourth it is larger by 0.4 d.v.; that in the major third it is larger by 2.5 d.v.; and that it is also larger by 4.0 d.v. in the major sixth. The musical *S*s have been exposed to and have become habituated to this divergence from the ratios more than have the non-musical.

What effect has this difference in habituation to changes in the equally tempered scale on the judgments of the two groups of *S*s? This question is answered, so far as our data are concerned, in the percentages of right judgments by the two groups given in the lower parts of the table. Note first the records of the musical *S*s. The interval of the fifth in the equally tempered scale is slightly smaller than the perfect fifth. Therefore it should be easier for the *S*s to recognize an augmented change in the perfect interval than a diminished change because the augmented change is the reverse of that to which they have become more or less habituated. The results agree with this supposition. In 7 cases of different degrees of modification out of a possible 9 the percentage right is higher on augmented than on diminished changes, while in only two is the reverse condition found. (Cases of no differences are counted ½ each way.) This gives a probability of 0.778 that training was influential, since otherwise the chances would be even for a positive or a negative difference. We have

TABLE V

PERCENTAGE CORRECT JUDGMENTS ON THE SEVERAL INTERVALS WHEN AUGMENTED AND WHEN DIMINISHED

Interval	Fifth ($c':g'$)	Fourth ($c':f'$)	Major Third ($c':e'$)	Major Sixth ($c':a'$)
Ratio in just temp't	2:3	3:4	4:5	3:5
Ratio in equal temp't	$1:\sqrt[12]{2^7}$	$1:\sqrt[12]{2^5}$	$1:\sqrt[12]{2^4}$	$1:\sqrt[12]{2^9}$
d. v. in just temp't	256:384.0	256:341.3	256:320.0	256:426.6
d. v. in equal temp't	256:383.6	256:341.7	256:322.5	256:430.6
Interval in equal temp't is	smaller, 0.4 d.v.	larger, 0.4 d.v.	larger, 2.5 d.v.	larger, 4.0 d.v.

	Interval change	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.
	10 d.v.	96	93	3	91	96	−5	87	90	−3	84	84	0
	8	92	92	0	88	92	−4	87	86	1	81	82	−1
	7	92	90	2	93	89	4	86	87	−1	76	82	−6
	6	86	84	2	86	86	0	62	79	−17	60	74	−14
MS	5	72	67	5	64	62	2	56	67	−11	44	54	−10
	4	47	42	5	47	49	−2	33	42	−9	31	40	−9
	3	32	31	1	28	36	−8	28	34	−6	26	32	−6
	2	26	27	−1	26	25	1	32	28	4	27	29	−2
	1	27	27	0	25	28	−3	25	26	−1	27	28	−1
		Ratio + to −, 7:2			Ratio − to +, 11:7			Ratio − to +, 7:2			Ratio − to +, 17:1		
	Prob'ty of infl. of training	Yes		0.778	Doubtful		0.611	Yes		0.778	Yes		0.944
												Mean	0.778
	10	62	56	6	59	53	6	53	46	7	46	47	−1
	8	57	57	0	71	61	10	58	53	5	52	47	5
	7	60	54	6	57	52	5	56	50	6	52	47	5
	6	52	54	−2	53	59	−6	44	52	−8	42	46	−4
NMS	5	42	42	0	40	44	−4	37	38	−1	30	37	−7
	4	33	27	6	34	30	4	30	33	−3	26	37	−11
	3	29	31	−2	30	33	−3	36	31	5	27	28	−1
	2	28	26	2	26	31	−5	32	26	6	26	26	0
	1	29	28	1	30	28	2	30	28	2	24	23	1
		Ratio + to −, 2:1			Ratio − to +, 4:5			Ratio − to +, 1:2			Ratio − to +, 11:7		
	Prob'ty of infl. of training	Slight or ?		0.667	No		0.444	No		0.333	Doubtful		0.611
												Mean	0.514

therefore listed this case ('yes') as showing the effect of habituation to equal temperament in the case of the music students. The effect is not so marked (probability of influence 0.667), and is listed as 'slight or doubtful,' in the case of the non-musical *S*s who show 6 in 9 cases of higher percentage right on the augmented changes.

In the case of the fourth, in which the equally tempered interval is slightly larger than the perfect fourth, we find the opposite effect in the percentages right by the MS group, but the effect is only slight ('doubtful'), the ratio of cases of higher percentages right on the diminished interval being only $5\frac{1}{2}$ in 9, or 11 in 18 (probability of influence 0.611). It is not present or slightly reversed in the NMS group (probability of influence only 0.444). These results are probably to be expected because of the small difference between the just and the equal temperament interval. Why the probability of influence by habituation should be so far below 0.500 we do not know.

The other two intervals, however, the major third and the major sixth, are considerably larger in the equally than in the justly tempered scale, the difference being absolutely greater in the major sixth than in the major third; and the results of the musical *S*s accord well with the degree of these two differences. In both cases the diminished changes effected in the experiment were more clearly recognized than were the augmented changes, but the probability of influence, as would be expected, is greater in the major sixth (0.944), whose justly tempered interval diverges absolutely more in the other direction, i.e. is augmented more, than it is in the major third (0.778). Since in both cases the *S*s of the MS group had become habituated more than had those of the NMS group to the augmented changes in equal temperament, they more readily noticed changes from the just temperament interval in the other direction, i.e. diminished changes, than changes in the same direction (augmented). This tendency is hardly present in the NMS group of *S*s, going for them one way in the major third (probability of influence 0.333) and another in the major sixth (probability 0.611). Why the former is so much below 0.500 we cannot say. In no case, of course, did any of the *S*s know the direction of change in the intervals to which they responded.

When we take into consideration all the judgments at different d.v. deviations from the ratio of the justly tempered interval in the fifth, the fourth, the major third, and the major sixth together, and note that if habituation to the equal temperament has no effect the chances are even that each group on each of the 36 (4 x 9) judgments will make a positive or a negative difference, we can determine the probability of influence by habituation in the case of each group. For the MS group this probability is 0.778 ± .0660,[1] and for the NMS group it is 0.514 ± .0795. The difference is 0.264 ± .1033, indicating a probability of about 958 in 1000 that the difference is not due to chance. Taking only the last two intervals (major third and major sixth), in which the change in equal temperament is larger, we get a difference of 0.389 ± .0965 (probability of 0.861 ± .0549 for the MS and of 0.472 ± .0794 for the NMS group), indicating a probability of about 999 in 1000 that the musical *Ss* were more affected by habituation than the non-musical. This difference reaches the point commonly regarded as statistically reliable.

From these data, involving a large number of judgments (9 x 10 x 18 = 1620, omitting 20 x 18 cases of no changes in just temperament intervals) on every interval by each group, the conclusion seems justified, that effects of habituation on reactions to consonance are perceptible even in the case of musical training extending over only a few years. These results[2] agree with those

[1]Cf. K. J. Holzinger, *Statistical Methods for Students in Education*, 1928, 248, formula 106.

[2]M. F. Meyer (The musician's arithmetic, *Univ. Missouri Stud.*, 4, 1929, 141-144), working on the basis of a report of this work which he evidently did not understand very adequately from the senior author's brief presentation, has commented interestingly on our results. While his suggestion of using the octave as one interval is good, he overlooks the fact that the fifth and the fourth are also practically unchanged in the equal temperament and that our differences in effects of habituation on the two groups of *Ss* are unreliable in the case of these intervals. This would evidently also be the case with the octave. We hope, however, to try this out in a further experiment including also other intervals. He has grossly misunderstood our report and abstract in taking us to mean that habituation is the only factor operating to make certain intervals 'acceptable.' He seems also to have overlooked an article on consonance by the senior author of this study (Joseph Peterson, A functional view of consonance, *Psychol. Rev.*, 32, 1925, 17-33), which should have corrected his erroneous understanding of our view. We, of course, do not regard Meyer's vague phrase "biochemical affinities of tones" as explanatory, though we recognize the value to psychology of much recent physiological research on the nature of the nerve impulse. Meyer's own 'applications' to consonance perceptions of ideas assumed from these researches cannot be taken seriously by anyone who is not satisfied with mere phrases like that mentioned, "the power of 2," etc. This 'power of 2' has been scientifically (*Continued on next page*)

obtained by Meyer[3] and by Moore.[4] Habituation during the life of an individual is evidently an important determinant of consonance in any musical interval. There is no desire to imply in this report that persons not explicitly trained in music are free from the effect of habituation here pointed out, but only that the training given the music students is such as to increase this effect. It is desirable to continue this investigation with *S*s working on such instruments as the violin and the 'cello and who have as little contact as possible with the equally tempered instruments.

Summary and Conclusions

Two groups of 18 college students in each, one group (MS) trained in music and the other (NMS) not, were individually taken through experiments in which they were to indicate which of four musical intervals (fifth, fourth, major third, and major sixth) was "slightly changed or unnatural." In every experiment one interval was either augmented or diminished to the extent of 1 to 10 d.v. in a chance order. Occasionally as a 'control' no interval was changed. The *S*s were permitted to omit a judgment and simply make a check mark whenever they did not notice any change. In individual records every fourth of these omissions was marked right, since by chance they could guess correctly 25% of the time. Every one of the 36 *S*s made 380 judgments.

The results show that for the MS group the ranges of unnoticed change in the fifth, the fourth, the major third, and the major sixth were $4.70 \pm .60$, $4.95 \pm .66$, $5.15 \pm .70$ and $5.74 \pm .76$ d.v. (or 29, 31, 32, and 36 cents), respectively, while for the NMS group it was over 10 d.v. on all intervals. A given absolute change was recognized in the first two intervals more readily than in the last two. Changes in the justly tempered intervals in the

(*Continued*) analyzed to a considerable extent by W. V. D. Bingham (Studies in melody, *Psychol. Monog.*, 12, 1910, (no. 50), 1-88). The successive presentation of our intervals *could* be employed in an experiment like this, it is true, but it would at first not be as precise in its bearing on the present problem, since it would involve slightly different factors. The acceptability of successive tones is certainly also conditioned by habituation, though not based fundamentally on this, as is also true of the simultaneous method we have employed. Meyer overlooks the reliability of the differences in judgments of our two groups.

[3]M. F. Meyer, Experimental studies in the psychology of music, this JOURNAL, 14, 1903, 207-214.

[4]H. T. Moore, The genetic aspects of consonance and dissonance, *Psychol. Monog.*, 17, 1914, (no. 73), 1-68.

direction opposite to those made in equal temperament were sensed more readily than changes in the same direction, especially by the musical *Ss*, who had become more fully habituated to the equal temperament than had the members of the other group. On the intervals most changed in equal temperament this difference in effects of habituation in the two groups is statistically reliable. Habituation is evidently an important factor in consonance perception, though of course not the only one.

THE EFFECT OF ATTITUDE UPON FEELING[1]

By E. Frances Wells, Bryn Mawr College

In the course of an extensive study of affective experience it was found that feeling varies markedly with the nature of the initial attitude or 'set' which the *O* adopts immediately before the stimulus is presented.[2] This led us to make an intensive study of the effect of certain specific attitudes upon feeling. The attitudes chosen for investigation were (1) the critical affective attitude, (2) the critical perceptive attitude, and (3) the naive or common-sense attitude.

Our experiments fall into three groups, distinguished by the type of instructions employed.[3] In Group I, we used a general instruction, requiring a description of the total experience from the moment preceding the presentation of the stimulus (initial attitude) to the termination of the affection. The observational attitude was not defined. In Group II, we used specific attitudinal instructions. The *O*s were required to adopt a particular observational attitude, as defined in the instructions. No instructions were used in the experiments of Group III. Experience was allowed to run its course without artificial determinations, and the *O* was then asked to give a retrospective report.

Group I includes 10 series of experiments, and 772 separate observations. 183 different stimuli were used. They were simple sensory stimuli of all modalities (Ser. I-VII); phonographic selections, including classical, sacred, and popular music (Ser. VIII); colored prints (Ser. IX); and an assortment of cartoons,

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[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Hoisington.

[2]The term *attitude* is used throughout this paper in the sense of a psycho-physiological set or predisposition toward a particular type of response. *Cf.* the 'sensory set' *vs.* the 'muscular set' in the reaction-time experiments.

[3]The experimental work was done at Cornell University between October 1927 and April 1928. The *O*s were Dr. L. B. Hoisington (*H*), Assistant Professor of Psychology; Mr. O. D. Anderson (*A*), Assistant in Psychology; Mr. G. L. Freeman (*F*), a graduate student; and the author (*W*). All of the *O*s had had considerable previous experience in psychological observation. Miss Emeline Moul kindly acted as *E* whenever the author observed throughout the experiment.

A detailed description of the experiments, with complete lists of the stimuli used, may be found in the original thesis, entitled *An Experimental Study of Affective Experience*, which is deposited in the Library of Cornell University.

comic pictures, jokes, simple problems, statements designed to arouse affirmation or negation, *et cetera* (Ser. X). The general procedure was uniform throughout this group of experiments. The details varied as the stimulus situation demanded. The stimuli were presented in haphazard order. *O* was seated in such a position that he could not see *E*'s manipulation of apparatus. Two signals were given before each observation, "Ready" and "Now." At the *ready*, *O* closed his eyes, except when the stimulus was visual, and prepared to observe. The stimulus was presented immediately after the *now*.

Group II includes 3 series of experiments, and 289 separate observations. A mixed group of 30 stimuli, selected from Ser. I-X (3 stimuli from each series), was used.[4] The instructions for the first series of the group (Ser. XI) required the adoption of a strictly common-sense attitude, the kind of an attitude with which one takes affective experience in everyday life. In Ser. XII, the instructions called for a purely perceptive attitude, an attitude of inquiry regarding the nature of the stimulus experience. In Ser. XIII, the *O*s were instructed to adopt their 'normal' attitude for the experiment. The 'normal' attitude for all 4 *O*s was a critical affective (or affective-sensory) attitude. The procedure in Ser. XII and XIII was essentially the same as in the experiments of Group I. In Ser. XI, the usual procedure was modified so as to facilitate the adoption of a common-sense attitude. With this end in view, the stimulus-situation was made as informal as possible. The *ready-now* signals were abandoned, and the stimuli were presented with such a statement as "I am going to give you a smell," or "I want you to look at this picture." Sometimes the stimulus was presented without any warning, taking *O* by surprise.

Group III consisted of 10 affective situations, so arranged that the *O* did not realize that he was being experimented upon until, during the course of the feeling, he was asked to report. His attitude was consequently naive.[5] 32 such reports were recorded.

Results

(*1*) *Variation of attitude under general instructions.* The *O*s' reports in the work under general instructions (Ser. I-X) show that their initial attitude—the set with which they awaited the stimulus—was by no means constant. It varied widely and unpredictably. When minor variations are properly subordinated, three main types of attitude emerge; the critical affective, the critical perceptive, and the naive or common-sense attitude. All of the attitudes reported represent either one of these 3 types or intermediate forms.

a. The critical affective attitude. The critical affective attitude is described as a set to react affectively to the stimulus, and

[4] The same group of stimuli was used in all 3 series except that the musical and ideational stimuli were varied so as to eliminate the effect of habituation.

[5] In one case, *e.g.*, *E* asked Observer *A* a question regarding one of his reports. While *O* was reading the report, *E* placed a piece of cold steel on the back of his neck. A moment later he was asked to report.

to observe the total experience critically. It is a set more for feeling than for perceiving. Under this set, the *O* is predisposed toward affective experience, more specifically toward affective *content* rather than affective *significance.*

b. The critical perceptive attitude. The critical perceptive attitude is a set to observe critically, to perceive rather than feel, to react perceptively instead of affectively to the stimuli presented. This set predisposes the *O* toward a clear-cut, non-affective perception of the stimulus-experience.

c. The common-sense attitude. The common-sense attitude is the attitude with which we meet affective experience in everyday life outside of the laboratory, as opposed to the attitude of psychological observation in an experimental situation. It is a 'natural' unrestricted set, a set to react naively to whatever comes without attempting to scrutinize experience. It is not critical. Such a set predisposes *O* toward affective significances and affective reactions rather than toward affective content. The common-sense attitude may be induced experimentally by appropriate instructions and procedure (Ser. XI).

(*2*) *Affective experience under the critical affective attitude.* Under the critical affective attitude, experience invariably develops as a pattern of qualitative content, describable in sensory terms. This pattern is usually affective (774 cases out of 809, or 96%).

a. Pattern characteristics. The pattern is typically a total fusion, without any clear-cut division into focus and background. Stimulus-experience and bodily experience tend to merge together, to fuse, more or less at one level of clearness. The degree of fusion varies. In the limiting case, the fusion is complete; there is absolutely no differentiation within the total.

Observer A. (10 I *P*)[6] It (the affective diffusion) became part of the odor experience; the two were inextricably bound up together. The affective experience was just as focal as the odor experience.

Observer F. (35 VII *P*) The bodily response was given as a totality, as homogeneous; diffuse bright pressures and a bright reddish glow all beaten up.

[6] The symbols which precede each quotation from Ser. I-X indicate (1) the period of observation, (2) the series number, and (3) the affective characterization of the pattern, (*P*, pleasant; *U*, unpleasant; *M*, mixed; or *O*, no affection). In quotations from Ser. XI-XIII and XVI, the serial number is followed by a second Roman numeral, which indicates the series to which the stimulus originally belonged. In quotations from Ser. XIV, the period of observation is not recorded.

Observer H. (3 I *U*) Experience lost its focus-and-background character. There seemed to be a single total without differentiation into focus and background.

Observer W. (31 VI *U*) The whole pattern merged together as usual, losing its sharp division into focus and background.

This total pattern has no specific localization. With the affective fusion, experience loses its specific space reference, and the resultant is an ill-defined mass, with only a vague center of reference or locus. This locus varies. It may be (1) bodily, referred vaguely to the body as a whole; (2) abdominal, referred to the abdominal region;[7] (3) stimulus, referred 'out there' in the direction of the stimulus-experience; or (4) transcendent, without spatial reference, not referred to any physical object.

Observer A. (37 VII ?) The total experiential pattern isn't any definitely localized 'out-thereness.'

Observer F. (27 III *P*) It seems to pervade the entire body; it is everywhere and nowhere in particular (bodily locus). (46 VI *P*) Bodily experience is included with the sound out there, slightly projected (stimulus locus).

Observer H. (15 II *U*) A mass of bodily pressure, very vaguely localized, yet somehow referred to the abdominal region (abdominal locus). (19 III *P*) All reference to things drops out and there is just sheer enjoyment; as we say, being carried away completely (transcendent).

Observer W. (30 VI *U*) The sound itself was no longer localized specifically. It was just out there in that general direction.

Although there is no invariable correlation between type of stimulus and affective locus, there are certain unmistakable trends. Intensely *U* stimuli normally give rise to patterns with abdominal locus. Of stimuli which are not intensely *U*, taste, smell and touch induce patterns with bodily locus, as a rule. With auditory stimuli, bodily locus is less frequent; stimulus and transcendent loci are more common. With visual stimuli, stimulus-locus is the rule; bodily and transcendent loci are infrequent.

b. Content. The content of the affective pattern includes stimulus-experience and bodily experience. The latter usually takes the form of the specific affective stuff: diffuse, loosely textured, vaguely localized, pressury-like experience; liveness, brightness, softness, or bright pressure, in *P;* heaviness, dullness, deadness, or dull pressure, in *U*.[8] This specific affective content diffuses through the total experience, altering it qualitatively and otherwise.

Observer A. (26 II *U*) Diffuse, bulky, heavy, voluminous stuff. This seemed to be infused into every bit of the total pattern.

[7] This type of locus occurs only with *U* feelings.

[8] *Cf.* Nafe's results. (J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544, *esp.* 508-517.)

Observer F. (19 II *P*) Experience became definitely brighter and livelier. The smell quality became more pressury, beaten up with the lively brightness.
Observer H. (49 IX *P*) It was suffused with a lively something like brightness.
Observer W. (34 VII *U*) The whole thing took on a dullish pressury tone. It was a rather loose, weak, dullish pressure.

c. Temporal course. The affective reaction usually occurs very soon after the presentation of the stimulus, before the stimulus-experience is fully perceived. It is as though the affective shift were touched off by the first hint of the nature of the stimulus experience. The development of the characteristic affective pattern follows immediately upon this shift. At the end, the affective pattern disintegrates, the feeling terminating in one of three ways; (i) affective stuff and stimulus experience drop out simultaneously; (ii) affection disappears before the stimulus-experience, and the *O* shifts to a perceptive attitude; or (iii) stimulus experience drops out before the affection, leaving an affective hang-over. In the latter case, the affection is usually referred to, or directed upon, the stimulus-experience that has disappeared. But, in a few cases, no such reference was observed; and the affection seemed to exist independently for a brief interval.

Observer A. (46 VIII *P*) A sort of carry-over in auditory-imaginal terms of the particular part of the piece that seemed to strike me as most *P*.
Observer F. (28 III *P*) It (the odor) dropped out completely, and, at that very moment, the affective pressures and their meaning dropped out also.
Observer H. (18 III *P*) A new qualitative resemblance appeared, and it switched right over (from affection) to perception of the odor. (40 VIII *P*) A *P*-like toning. It had practically no objective reference. The idea of the music did not come in at all.
Observer W. (27 V *P*) The bright liveliness of bodily experience remained for some little time afterwards. There wasn't any reference to the stimulus-experience that had been there. The *P* wasn't referred to anything.

(*3*) *Affective experience under the critical perceptive attitude.* Under the critical perceptive attitude, affective experience simply does not occur. The attitude excludes feeling. Experience develops as a perceptual pattern, sharply divided into two levels, a clear focus and an obscure background.

a. Affective reaction with shift of attitude. Potent affective stimuli frequently induce a shift of attitude and consequent affective reaction. Our *O*s found that it was practically impossible to maintain a constant perceptive attitude, as demanded by the instructions (Ser. XII), over against such stimuli as hydrogen sulphide, the tickle of a feather, and music, which somehow compel an affective reaction.

In 35% of the observations under instruction for perceptive attitude, an affective response occurred. In every case, the *O* reported that he had failed to maintain the required attitude.

b. Characteristics of the affective reaction. When an affective reaction occurs in spite of the initial perceptive attitude, it comes, usually, as the end-phase of a fairly complete perceptual development. The stimulus is fully perceived, reacted to perceptively, before the shift to affection. The affective pattern is incompletely fused, as a rule; the feeling is relatively weak and of short duration, generally terminating with a shift back to the original perceptive attitude before the stimulus experience is gone.

(4) *Affective experience under the common-sense attitude.* Under a common-sense attitude, such as our *O*s assumed in Ser. XI and XIV, affective experience occurs practically as frequently as it does under the critical affective attitude (185 out of 194 cases affective, or 95%). But the nature of the affective reaction differs radically. Under a common-sense attitude, affective experience comes as a patterned reaction rather than as a pattern of qualitative content. The content of the pattern is too obscure to be observed in detail, and it is the meaning, the significance, of the reaction that is the dominant aspect of the total.

The reaction pattern may be described as either positive or negative; a pull forward, *attraction*, or a pull backward, withdrawal, *repulsion*. In the one case, the *O* is attracted toward the stimulus-experience; he is 'open' and receptive toward it. Frequently he tries to get more of it by moving toward the stimulus, breathing more deeply, *etc.* In the other case, he is repelled by a stimulus-experience; he shrinks away from it, attempts to escape from it, by inhibiting breathing, withdrawing the part stimulated, removing the stimulus, or by general bodily withdrawal.

Under this attitude, bodily experience does not have a definite qualitative content. At best, it is vague undeveloped stuff; there is nothing as specific as bright pressure or dull pressure in it. The most that the *O* can say is that there is some stuff in the background of indefinite nature, with perhaps a hint of brightness-dullness.

Observer A. (53 XI-V *U*) The thing was *U* common-sensically in the same way that a fly crawling over one's forehead is *U*. The typical pattern of the stuff wasn't there at all. The thing was irritating, annoying, and that is all I can say about it.

Observer F. (52 XI-III *U*) That was affective in the sense of being disturbing. I don't know whether it was bright or dull. There was a bodily reaction, but I didn't get any of its qualities.

Observer H. (XIV *P*) The whole experience focussed upon the roses, something of a pull toward them. They were beautiful. When I smelled, the odor came as more than just the odor of roses; it was 'that delightful odor.'

Observer W. (53 XI-I *P*) That was a *P* smell. Again, I didn't notice any bodily change when experience became affective. It was simply that the smell was *P*, that I liked it.

The effect of the common-sense set upon the temporal course of affective experience seems to be primarily negative in character. The course of experience varies widely and unpredictably, apparently determined by factors quite apart from the initial attitude. No positive influence is manifest.

Our results show quite clearly that the attitude with which one approaches an affective situation has an important effect upon one's reaction to it. In fact, it determines to a large extent what that reaction is to be. It determines, not only whether one is to react affectively or non-affectively, but also whether the pattern is to be one of qualitative content primarily (sensory stuff), or of reactive content primarily (significant movements and meanings).

The results of the work under the common-sense attitude indicate at least one important reason for the unsettled state of affective psychology; they show why feeling has gone undescribed for so long. When feelings are stripped of the naive common-sense attitude, in which they normally have their being, and are observed under a critical affective attitude, they are reduced to modes of tactual experience; to pressure or something very much like pressure. Such pressures may be regarded as the laboratory equivalent of our normal feelings, but experientially they are far removed from the feelings of our everyday life; they are not feelings in the sense that that term is generally understood. Rather they are laboratory artifacts, products of a specific unnatural observational attitude, yet none the less psychologically genuine.

When we once recognize the importance of attitude as a determinant of affective experience, there need be no incongruity in admitting that the 'affective pressures' found in the laboratory do not occur in everyday life. Indeed, we should not expect to find them there. What we find, in any given case, will be a product of the attitude under which we make the observation.

Summary

1. When observing affective experience under a general instruction to describe the total, the *O*'s initial attitude tends to vary widely and unpredictably.

2. Three types of initial attitude are described; the critical affective, the critical perceptive, and the common-sense attitude.

3. The critical affective attitude is a set to react affectively to the stimulus and to observe critically the total experience.

4. The critical perceptive attitude is a set to observe the stimulus-experience critically without reacting affectively to it.

5. The common-sense attitude is the set with which we take experience in everyday life as opposed to the critical laboratory set.

6. Under the critical affective attitude, experience usually develops as an affective pattern in which qualitative content predominates. This pattern is typically a total fusion without clear-cut division into focus and background or specific localization.

7. Under the critical affective attitude, the content of the affective pattern usually includes diffuse pressure-like experience; affective stuff, which diffuses through the total; bright and lively in *P;* dull, dead, and heavy in *U*.

8. Under the critical affective attitude, the affective reaction usually occurs very soon after the presentation of the stimulus. At the end, the affective stuff may drop out before the stimulus-experience, after the stimulus-experience, or simultaneously with it.

9. Under the critical perceptive attitude, affective experience does not occur. Experience develops as a perceptual pattern, sharply divided into a clear focus and an obscure background.

10. When an affective reaction occurs under instructions for a critical perceptive attitude, there is always a shift away from the perceptive attitude. In such cases, the feeling usually occurs late in the course of experience and drops out before the stimulus experience.

11. Under the common-sense attitude, affective experience occurs as a patterned reaction, in which the content aspect is too obscure to be observed in detail and the meaning or significance of the reaction dominates. Bodily experience comes as vague undeveloped stuff. There is nothing as specific as bright pressure or dull pressure in it.

12. These results show that the attitude with which one approaches an affective situation plays an important part in determining one's reaction to it. The nature of the affective reaction varies widely under different attitudes.

THE MEASUREMENT OF TONUS BY DEFORMATION OF THE TENDON

By G. L. FREEMAN, Yale University

Psychologists and physiologists recognize the importance of a reliable indicator of muscular tonus in the intact human organism. The extensive literature on the subject indicates, however, that the results obtained by most available techniques are not susceptible of precise quantitative treatment. The trial of several of the existing means of record led the writer to exploit a new indicator which he wishes to describe.

Before dealing with the new method and apparatus, it may aid the reader to recall the several ways in which muscular tonus has been heretofore recorded. There are two types of indicators, mechanical and electrical. Two different mechanical systems have been used; the one records the movement of the mobile limb which is attached to the tendon of the contracting muscle, the other records the thickening of this muscle. The only electrical phenomenon which may be safely correlated with muscular contraction at present is the electromyogram. Each of these methods may be briefly discussed.

Mechanical systems. The earliest means of recording changes in tonus involved the use of the tendon reflexes, notably the knee-jerk. Periodic blows were delivered to the tendon and the movement of the mobile limb was recorded. Since the resistance offered by the tendon is dependent upon the tone of the attached muscle, variations in the extent of the limb-movement were held to be an index of tonus-change. Such a method has been used extensively by Muskens,[1] Lombard,[2] Tuttle,[3] and many others.

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[1]L. J. J. Muskens, Muskeltonus und Sehnenphänomene, *Neur. Centralbl.*, 18, 1889, 1074-1086.

[2]W. P. Lombard, The variations of the normal knee jerk and their relation to the activity of the central nervous system, this JOURNAL 1, 1887, 2-71.

[3]W. W. Tuttle, The distribution of tone in skeletal muscle, *J. Exper. Psychol.*, 8, 1925, 319, 322.

Similar methods, *e.g.* those of Golla[4] and Travis,[5] record the slight movements of the limb due to increased tonus without evoking the reflex. Another variation, introduced by McKinley and Berkwitz,[6] measures the torque around the elbow-joint as the muscles of the arm sustain various weights.

Since the publication of Dodge's[7] systematic exploration of the knee-jerk, muscular thickening has been used extensively as an indicator of isometric contraction. While this is a more indirect measure, it has certain advantages over records obtained from a mobile limb, notably the escape from the momentum and inertia of the member. A common system of recording thickening uses two Marey tambours connected by an air column.[8] The receiving tambour is placed over the antagonistic muscle and the recording tambour marks on smoked paper. Such systems distort the curves by their inertia and momentum and by the elasticity of the air column. Moreover, protracted fine adjustment is difficult. Somewhat more satisfactory results are obtained by placing a lever over the muscle and photographing its excursions. There are two main limitations to the use of muscle-thickening as an indicator of tonus, (1) the small changes that are produced and (2) the fact that with high magnification these small changes may be masked by pulse and respiration.

Electrical systems. Following Piper's[9] monograph on the electrophysiology of human muscle, the electromyogram has been increasingly useful in the study of muscular contraction. Electrical indicators of tonic contraction cannot, however, be successfully exploited at present. A characteristic electromyogram of voluntary contraction may be obtained from two zinc electrodes placed on the surface of the arm and connected with a string galvanometer which records on a rapidly moving film. The interpretation of the record is simplified, since the significant facts are those of a rapidly oscillating rather than of a steady current. So long as no attempt is made to interpret the constant deflections

[4]F. L. Golla, The objective study of neuroses, *Lancet*, 201, 1921, 118.
[5]R. C. Travis, A study of muscle-tonus and its relation to fatigue, *J. Exper. Psychol.*, 7, 1924, 202 ff.
[6]J. C. McKinley and N. J. Berkwitz, Quantitative studies of muscle tone, *Arch. Neurol. and Psychiat.*, 19, 1928, 1036-1056.
[7]R. Dodge, A systematic exploration of the normal knee jerk, *Zsch. f. allg. Physiol.*, 12, 1910, 1-58.
[8]F. L. Golla, *op. cit.*, 119.
[9]H. Piper, *Elektrophysiologie menschlicher Muskeln*, Berlin, 1912.

of the string, it is quite permissible to use any sort of electrodes, including needles placed directly in the muscle. Such needle-electrodes, first introduced by Rehn,[10] have the advantage of producing a better localized electromyogram.

Recently Lewy[11] (using plate electrodes) and Golla[12] (using needle electrodes) have presented evidence that tonus changes are correlated with the *constant* deflections of the galvanometer-string. These observations are intimately related to the whole question of the possible sympathetic innervation of tonic contraction. This question has been made the subject of exhaustive review by Adrian,[13] Fulton,[14] and Forbes,[15] all of whom conclude that the data do not justify the conclusion. The mass of physiological evidence shows that, with favoring conditions and suitable amplification, small electrical variations of an oscillatory character will be found to accompany tonic contraction. That oscillations have not always been observed with even the most sensitive recorder[16] may be due to the possibility that the action currents involved were in opposite directions. Since the use of pad electrodes gives only the average current for the whole muscle group, the galvanometer-string need not be set in oscillation if the innervation of this group is sufficiently asynchronous.

Records of constant deflections are inconclusive evidence of muscle-tone chiefly because similar deflections are also produced by polarization-changes in the skin, in the electrodes and—in the case of needle electrodes—in the muscle fluids. But there are yet other sources of error. Cobb[17] has exhibited records quite indistinguishable from those of Lewy, which were produced by a

[10]E. Rehn, Elektrophysiologie krankhaft veränderter menschlicher Muskeln, *Dtsch. Zsch. f. Chir.*, 142, 1921, 155 ff.

[11]F. H. Lewy, Die Lehre vom Tonus und der Bewegung, 1923, Springer, Berlin.

[12]F. L. Golla, *op. cit.*, 119.

[13]E. D. Adrian, Oliver Sharpey lectures on the interpretation of the electromyogram *Lancet*, 208, 1925, 1229-2344, 1282-1286.

[14]J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 383-426, Baltimore, 1926.

[15]A. Forbes, Tonus of skeletal muscle in relation to sympathetic innervation, *Arch. Neurol. and Psychiat.*, 22, 1929, 247-264.

[16]C. Liljestrand and R. Magnus, Über die Wirkung des Novokains auf den normalen und den tetanusstarren Skelettmuskel und über die Entstehung der lokalen Muskelstarre beim Wundstarrkrampf, *Pflügers Arch. f. d. ges. Physiol.*, 176, 1919, 168-208.

[17]S. Cobb, Electromyographic studies of paralysis agitans, *Arch. Neurol. and Psychiat.*, 8, 1922, 247, 264. See also Review of the tonus of skeletal muscle, *Physiol. Rev.*, 5, 1925, 518-550.

passive movement of the electrodes. And Einthoven[18] has shown that similar deflections may be obtained by the passive movement of a completely denervated muscle.

In summary, then, the oscillatory character of an electrical response is probably the only fact at present acceptable as an indicator of tonic contraction. The slight nature of these responses usually requires triodic amplification. Even assuming the great care which the use of such systems necessitates, the records obtained cannot be treated in a quantitative manner. Frequency and amplitude of the oscillations are the only variables which might be correlated with the force of contraction. Adrian[19] states that frequency is apparently determined by the "natural tendency of the motor neurones to discharge at the rate of 50 a second when their excitability is raised by a cortical effect" and that the amplitude of the oscillations "may depend upon many factors which are not necessarily related to the force of contraction."

The relation of tendon-deformation to muscular contraction. This survey convinces one that the mechanical response of tonically contracting muscle is more susceptible to measurement in human subjects than the electrical. The problem of measuring changes in tonus by mechanical means resolves itself into one of optimal magnification with a minimum of distortion. A likely solution is found in the relation of tendon-deformation to muscular contraction. When the tendon attached to a muscle is depressed by a constant pressure, changes in its deformation will be related to increments in muscular tension. These changes will theoretically be most extensive for the lower range of tensions. The most important difficulty in the exploitation of this method is that the quantitative relation of tendon-deformation to muscle-pull has never been established.

The immediate subject of our exploration was to discover the relationship between patellar tendon-deformation and quadriceps tension. The experiments were carried out mainly on a single subject. The results obtained, however, were not only regular in themselves but also correlated closely with occasional records obtained from two other subjects and with theoretical

[18]N. Einthoven, Sur les phénomènes electriques du tonus musculaire, *Arch. néer. de physiol.*, 2, 1919, 489-499.
[19]E. D. Adrian, *op. cit.*, 1231, 1286.

values. This suggests that the relationship found in the present exploration may be expressed as a law valid for all tendon-muscle groups of all individuals.

Theoretical relationship. Anatomically regarded, the patellar tendon is a *cord* attached to a fixed point (the tibia) at one end and to the quadriceps muscle at the other.[20] The tendon works

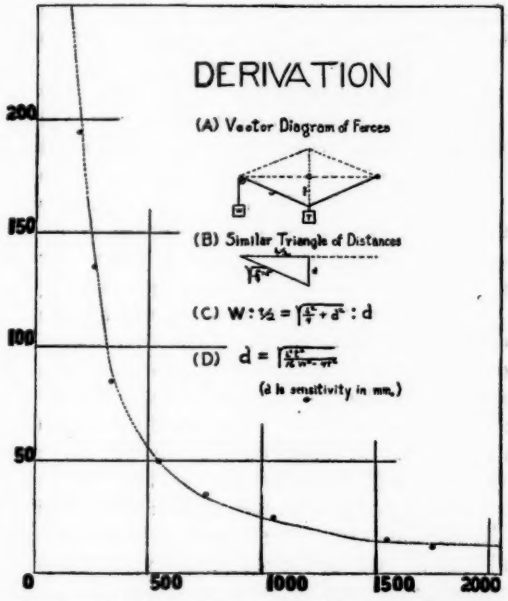


FIG. 1. SHOWING THE SENSITIVITY OF THE DEFORMED CORD FOR VARIOUS PRESSURES. THE DOTS ALONG THE CURVE REPRESENT VALUES OBTAINED IN THE EMPIRICAL VERIFICATION

over the knee-cap, which serves as a simple pulley in the system. When the tendon is depressed we have a system which resembles the theoretical construction of Fig. 1. In this figure and the corresponding formula, T represents the constant weight on a cord and W the pull of a variable weight exerted over a pulley, p. Any increment in pull at W raises T and so lessens the deformation, d. Any deformation of the cord will, in theory, bear a certain mathe-

[20] We shall assume in this treatment that the muscle is pulling directly on the tendon; although the actual pull of the muscle fibers is oblique.

matical relation to the amount of stress exerted at W. This relationship is expressed in the formula $d = \sqrt{\frac{L^2 \ t^2}{16W^2-4t^2}} \cdot L$ is a constant representing the length of the cord. (In the case of the patellar tendon it would be the distance between the point where the tendon is attached to the tibia and the axis of the knee-cap pulley.) The derivation of this formula is given in Fig. 1, as well as a theoretical curve. This curve represents the changes of a depressed cord effected by various weights when T equals 200 grams and L equals 50 centimeters. The heavy dots along the curve give the values obtained in an empirical test of the formula.

As the pull at W approaches zero, d approaches infinity. And as W approaches infinity, d approaches zero. The point of zero sensitivity for the curve in Fig. 1 is approximated when W is 2000 grams. Any further increase in the upper range of weights will have a negligible effect on d. Since, however, increase in the lower range of weights results in a disproportionately extensive movement of T along d, the system of the deformed cord should register the effects of slight pulls to great advantage.

Technical considerations. In our theoretical treatment we have assumed the tendon to be a freely moving cord. This is, however, not exactly the case. Pressure of a constant weight on the tendon establishes an elastic equilibrium not only against the tension of the attached muscle but also against the elastic tissues underlying the tendon. This elastic equilibrium would vary with changes in the tension of the muscle (after the manner expressed in the formula) only in case we could assume that the pressure of the cutaneous and subcutaneous tissues remains constant throughout and that the limb does not move.

Apparatus

Deformation of the patellar tendon was produced and its changes photographically recorded in the following manner. The subject sat in a chair with the right leg stabilized at the knee and with the tibia at right angles to the thigh. The foot was placed upon a movable platform from which an offset rose in the form of a stirrup. The stirrup offset rested on the platform with knife-edge joints, and was held against the tibia by constant pressure. This stirrup carried an adjustable axis which formed the fulcrum

of a lever resting against the patellar tendon. Deformation of the tendon was produced by pressing the lower end of the lever against the skin overlying it. This pressure was approximately 500 grams. A mark was made on the skin below the knee and the pressure was always applied at approximately the same point. Changes in the tendon were photographically recorded by magnifying the movements of the lever which depressed it as follows. A thread attached to the lever at the point of contact with the tendon encircled a cylinder of small known diameter (10 mm.) carrying a convex mirror of suitable curvature. The thread was held under constant tension by a rubber band. A recording camera was placed approximately 17 cm. from the mirror. The changes in tendon-deformation were thus magnified 68 times. (Magnification equals the distance from the camera divided by the radius of the cylinder multiplied by 2.)

Data on the relation of tendon-deformation to muscle-pull were obtained by having the subject raise various weights attached in the rear of the movable platform. This platform was held in position by a stop with the lower limb at right angles to the quadriceps. In the experiments the subject pushed the platform away from the stop just enough for the muscle to support a given weight. An electric circuit, including a Becker marker, recorded this moment on moving photographic paper. Since the measurement was taken at the beginning of the movement, the error from motion of the limb was negligible.

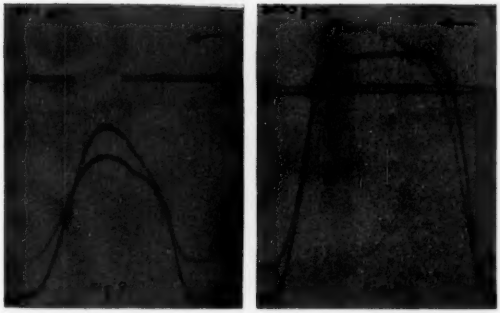
Friction of the system through which the pull of the quadriceps acted was reduced to a minimum, as follows. Three ball-bearing castors under the platform moved over a surface of polished brass. The cord which carried the weights ran over two pulleys with finely machined cone-bearings. The movement of the platform was kept in a straight line by means of two horizontal levers, of equal length, attached by bearings to the platform at one end and to a parallel block at the other. In order to obtain equivalent excursions in the records of quadriceps-thickening, an independent recording system was used, modeled after that described by Dodge and Travis.[21] It magnified the changes in thickening about 15 times.

[21] R. Dodge and R. C. Travis, The relationship between muscle pull and muscle thickening, this JOURNAL, 42, 1930, 295.

The Experimental Results

1. Comparison of recorded changes in muscle-thickening and tendon-deformation. It will be seen from inspection of the records (Figs. 2, 3) that, under the conditions described above, the form of the two curves is much the same. Both begin at approximately the same time (there is an apparent lag of less than 1/50 sec. in the quadriceps record). Both excursions reach a maximum at about the same time, though the tendon record seems to be somewhat more accelerated. Both events have approximately the same period of recovery. It is safe to assume, therefore, that the changes in tendon-deformation, as recorded by our technique, accurately reflect the changes in muscle-contraction as indicated by a lever resting on the muscle. It should be noted that there is an interval of time elapsing between the onset of the records of strain and the lifting of the weights as indicated by the movement of the Becker marker. That is to say, the weights are lifted only when the strain reaches a certain magnitude.

2. Comparison of the two records for various known pressures. The quadriceps muscle was made to lift weights of 500, 1000, 1500, 2000, and 2500 grams, attached to the foot as previously described. Two hundred records for each weight were taken over a period of several days. Distortion due to progressive changes in subcutaneous deformation was distributed as evenly as possible over all records. The deflections corresponding to tendon and muscle changes were read from the base line to a point on the records in line with the 'break.' The averages are given in Fig. 4. These deflections (in mm.) are indicated on the ordinates of the graph and the several weights are represented along the abscissa. The graph of muscle-changes shows that thickening increases in approximate direct arithmetical proportion to increments of weight. Although the graph of tendon-changes is the reverse of the theoretical curve (Fig. 1), the relation expressed is the same. The reason for the reversal is clear. The theoretical curve gives the total deformation of the cord for various weights, whereas Fig. 4 gives the changes in deformation from a depression produced in the tendon by a pressure of 500 grams. If Fig. 1 were turned 180° and the theoretical curve read from right to left we should have analogous curves. The relationship of deformation to pressure-increments is the same, therefore, regardless of



FIGS. 2 AND 3. DEFORMATION OF TENDON AND THICKENING OF QUADRICEPS WITH WEIGHTS

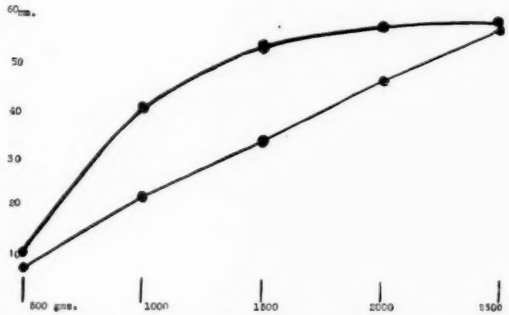


FIG. 4. AVERAGE CHANGES IN TENDON (UPPER CURVE) AND QUADRICEPS RECORDS WITH EQUAL WEIGHT INCREMENTS

Weight	Tendon change	Quadriceps change
500 gm.	11.1 mm.	7.2 mm.
1000	41	22.2
1500	54.2	34.3
2000	58.4	47
2500	60	58.5

FIG. 5. TENDON AND MUSCLE RECORDS WITH MINIMAL CONTRACTIONS

FIG. 6. TENDON AND MUSCLE RECORDS WITH MENTAL ARITHMETIC

whether the curve is drawn from the zero position of the cord or from a point of initial depression. Fig. 4 indicates that tendon-deformation changes unequally with equal increments in pressure. The greatest change is between the pressures of 500 and 1000 grams. It is presumptively still greater for lower pressures, *i.e.*, the rate of change increases progressively in a manner analogous to the theoretical curve.

It will be noted that the graph of quadriceps-thickening and that of tendon-deformation do not approach the same zero position. This is due in part to differences in optical magnification of the two systems. Since such differences are constant for all readings taken, we may safely infer that, wherever the two graphs are approximately parallel, the sensitivity of the systems is the same. Tendon-deformation, as measured by our technique, is a more sensitive index of muscular contraction than is quadriceps thickening only for pressures below 1000 grams. Pressures above 2500 grams will register little or no effect since above this point the tendon-deformation is not measurably changed.

We feel that the theoretical and empirical curves are sufficiently analogous for us to venture a tentative formulation of the following law. Decreases in tendon-deformation which accompany increases of strain in isometric muscle follow the formula of the deformed cord, subject to a correction (K) for the resistance offered by subtendinous pressures. The change in deformation is most rapid for small pressures and least rapid for large. Pressures above 2500 grams will register little or no effect, since around this point the tendon is no longer noticeably depressed.

3. Minimal contractions. In order to demonstrate the extreme sensitivity of the tendon record for small muscular contractions, a comparison was made between the changes in quadriceps-thickening and tendon-deformation at this level of strain. The lever-system placed over the quadriceps was made to conform to the exact design and measurement of that producing the tendon-deformation. Movements of the two levers were given, by approximate means, the same optical magnification. The subject alternately relaxed and tensed slightly the quadriceps. Fig. 5 reproduces a record obtained under these conditions. The reader should note the relative freedom from pulse of the tendon record in spite of its extreme sensitivity. This is an important advantage.

4. The application of tendon-deformation to psychological operations involving tonus-changes. Fig. 6 shows the increase in tonus (as recorded by our tendon technique) during the solution of a problem in mental arithmetic. The 'make' movement of the Becker marker indicates when the experimenter began dictation of the problem. The optical magnification of this record is 58. The line marked 'c' reproduces the position of a small mirror on the stirrup offset and indicates that the changes in the tendon record were not caused by a mechanical movement of this apparatus. Records such as Fig. 6 suggest a wide field of application of our technique in psychological studies of effort, fatigue, *etc.*

Summary

Our exploration of the measurement of tonic contraction by the method of tendon-deformation leads to the following tentative formulations.

1. With a constant weight pressing on the patellar tendon, decreases in its deformation are in some direct relation to increases in contraction of the quadriceps.

2. The nearest theoretical approximation of the relation of tendon-deformation to muscle-pull is found in the formula for the deformed cord, which was empirically verified by the author. Before this formula ($d = \sqrt{\frac{L^2 \quad t^2}{16W^2\text{-}4t^2}}$) could be successfully applied to tendon-deformation it would have to be corrected for changes in cutaneous and subcutaneous pressure, the exact nature of which we have no independent means of measuring.

3. Records of tendon-deformation offer an extremely sensitive indicator of small muscular contractions, particularly those known as tonus-changes. The effect of pulse on these records is minimal. The system does not recommend itself, however, to the study of gross muscular contractions.

4. The use of tendon-deformation for the study of tonus-changes occurring during mental effort, fatigue, *etc.*, is practicable; but quantitative interpretation of these changes is dependent on careful experimental controls.

WHOLE AND PART METHODS WITH UNRELATED REACTIONS

By Leland W. Crafts, New York University

The object of the present experiment was to compare, in massed and spaced practice, the efficiency of whole and part methods of learning when materials were used that were both serially and spatially unrelated. We selected materials of this type in order to test the proposition that the value of the whole method of learning varies directly with the coherence and unity of the materials learned—a proposition which is implied in assertions as to the superiority of the whole method for "getting the meaning of the whole,"[1] or for seeing "the connections of the several parts,"[2] and is experimentally supported by the studies of Sawdon[3] and Winch[4] on the learning of poetry.

With the exception of a few experiments with paired associates[5]—the results of which, incidentally, disagree—all of the experiments on the whole and part methods of learning have dealt with serial materials,[6] in which the reaction-elements, since every response becomes the stimulus for the next following response, are closely integrated. Because of this, the author repeated the experiment with non-serial materials, *i.e.* with a card sorting test. The results of this study, which have recently been published,[7] clearly showed that the whole method of learning was, in spite of the fact that serial relations were eliminated, superior to any of the part methods employed.

*Accepted for publication February 17, 1930.

[1] A. I. Gates, *Elementary Psychology*, rev. ed., 1928, 364.

[2] R. S. Woodworth, *Psychology*, 1921, 364.

[3] E. W. Sawdon, Should children learn poems in wholes or in parts? *Forum Educ.*, 5, 1927, 182-197.

[4] W. H. Winch, Should poems be learnt by school-children as 'wholes' or in 'parts'? *Brit. J. Psychol.*, 15, 1924, 64-79.

[5] L. A. Pechstein, Whole versus part methods in learning nonsensical syllables, *J. Educ. Psychol.*, 9, 1918, 381-387; Warner Brown, Whole and part methods in learning, *ibid.*, 15, 1924, 229-233.

[6] With poems, lists of nonsense syllables, paragraphs of prose, mazes, and the like.

[7] L. W. Crafts, Whole and part methods with non-serial reactions, this Journal, 41, 1929, 543-563.

The card sorting test, however, involved spatial relationships; consequently the conclusion could not be drawn that the superiority of the whole method did not depend upon the apprehension of relationships. It seemed desirable, therefore, to continue the study and to employ a type of material which lacked not only serial but spatial relationships as well.

A simple letter-number substitution test was selected, which was non-serial insofar as it did not permit the development of serial reactions. The final responses to be learned were the writing of certain numbers opposite certain letters. As far as the subjects were concerned the order of the letters on the test sheets, and consequently the order of the responses to them, was entirely a matter of chance. One reaction could not, therefore, function as the stimulus for another. The reactions, furthermore, were definitely non-spatial.[8]

The first problem of the present experiment being, therefore, to determine the values of whole and part methods for the learning of responses which are unrelated both serially and spatially, the second problem was to compare the efficiency of whole and part methods for the learning of such responses under massed and spaced conditions of practice. The most important study relevant to this latter inquiry is the well-known monograph of Pechstein's.[9] Using as his material a "very difficult" maze he found the whole method to be superior when the practice was spaced but part methods to be superior when it was massed. His results are no doubt valid. But since they are based on maze learning only, and since we have on this problem only one other experiment—that of Gopalaswami's[10] on mirror-drawing—which although in general confirmatory of Pechstein's is unfortunately very similar to it in the type of reaction employed, it would seem worth while to see if similar results would be obtained with the unrelated material selected for the present experiment.

[8]The movements involved, during the early stages of learning, in turning the eyes to the key or code, may have been guided in part at least by spatial considerations, but as learning progressed those movements became less and less frequent until finally they were entirely omitted. The final number-writing response, *i.e.* whether *S*, for example, wrote a '4' or a '5,' was in no way influenced by them. We feel justified, therefore, in stating that the reactions were not only non-serial but that they were non-spatial as well.

[9]L. A. Pechstein, Whole *vs.* part methods in motor learning: a comparative study, *Psychol. Monog.*, 23, 1917, (no. 99).

[10]M. Gopalaswami, Economy in motor learning, *Brit. J. Psychol.*, 15, 1925, 226-236.

Procedure

Subjects. The *S*s were 281 undergraduate men and women. They were all students of psychology and were drawn mainly from the classes of the writer.

Materials. The materials used were mimeographed sheets. In addition to the instruction and preliminary exercise sheets described below, every *S* received 10 experimental sheets bearing at the top all, or a part of, the following key:

a	b	c	d	e	f	g	h	i	j	k	l
4	10	7	1	9	2	11	5	12	8	3	6

Below this key were 16 horizontal rows of 12 letters each. The letters were arranged by chance except that identical letters were not allowed to come together, and that the same number of every kind of letter appeared in the successive pairs of rows.

The preliminary exercise sheet was identical with the experimental sheets described above except that the following letter-letter key was employed:

a	b	c	d	e	f	g	h	i	j	k	l
p	v	s	m	u	n	w	q	x	t	o	r

Instructions. The following instructions were given the *S*s:

"You will receive sheets of paper at the top of which will be printed letters and below every letter a number. For example: y = 28, z = 41. This constitutes the key or code. Below this will be printed in horizontal rows the letters of the key in a mixed order.

"The work of the test is to write to the right of every letter in the rows the number which goes with that letter in the key. For example, if you were using the sample key above you would write the number '28' after every 'y,' and '41' after every 'z.'

"When the signal to start is given, turn the first sheet face up and begin to work at once. Try for speed. Your score is the number right in the time allowed, 2 min. If you make an error, go on to the next letter without delay; you only lose time if you try to make corrections.

"Begin work at the left of the first row, work across that row, pass to the left of the second row, work across that row, and so on. Take every letter as it comes in the row. Do not attempt any grouping of letters, such as doing 'a' first. If you finish a sheet before the time is up, go on at once to the next one. When the signal to stop work is given, stop immediately."

The *S*s were also informed that the main experiment would be preceded by a 2 min. practice with a letter-letter key. They began at the word 'go,' and stopped at the word 'stop.' All were given the same instructions regardless of the method of practice which they were later to employ.

Amount and distribution of practice. The time allowed for work on every sheet, including the preliminary, was 2 min. The interval between successive sheets was 1 min. Time was kept with a stop-watch.

In the massed practice all the 11 sheets were allotted to a single sitting. In the spaced practice the distribution was as follows: The number of days on which practice was given was 3. The length of the intervals between them was 48 hr. Hence the total time covered by the practice was 5 days. The lengths of period (omitting the 1 min. interval between sheets) were 8, 6, and 8 min. on the first, second, and third days of practice respectively. In terms of

practice sheets the distribution was specifically as follows: on the first day the preliminary exercise and sheets 1, 2, and 3; on the second day sheets 4, 5, and 6; on the third day sheets 7, 8, 9, and 10.

Scoring. The score on every sheet was the number of substitutions made. Errors were so infrequent that no attempt was made to record them.

Practice Methods

(*1*) *The whole method.* For the *S*s who worked with the whole method all of the 10 practice sheets bore the entire 12-letter-12-number key, and the letters below it, for which the numbers were to be substituted, were equally divided among the 12 key letters.

(*2*) *The pure part method.* In the pure part method every one of the first 6 sheets bore only a part of the 12-unit key. On some sheets the first four letters (a, b, c, and d) with their appropriate numbers were given; on other sheets the second four letters (e, f, g, and h) and their numbers were given; and on still others the third four letters (i, j, k, and l) with their numbers. The letters in the 16 horizontal rows for which numbers were to be substituted were of course only the four which appeared in the part of the key given. The different part keys occupied the same position on the sheets that they would have occupied had the entire key been presented, *i.e.* if a sheet bore the first four units of the key they were placed to the left of the center of the sheet.

Practice with the above parts was distributed as follows. (a) For the massed practice: on sheets 1 and 2 (after the preliminary) the first part was presented; on sheets 3 and 4 the second part; and on sheets 5 and 6 the third part. (b) For the spaced practice: on the first day, on sheets 1, 2, and 3, parts 1, 2 and 3; on the second day, on sheets 4, 5, and 6, parts 1, 2, and 3 also. In both kinds of practice the parts were successively presented to the *S*s in either a left to right order, *i.e.* first 4, second 4, and third 4 units of the original key, or in a right to left order reversing the above. Each of these two orders was used approximately an equal number of times. Sheets 7 to 10 inclusive for the last 4 trials were identical with those throughout for the whole method, *i.e.* they bore the entire 12-unit key.

(*3*) *The combination part method.* In the combination part method sheets 1, 2, and 3 bore different 4-unit parts identical with those used in the pure part method. Sheets 4, 5, and 6 bore different 8-unit parts composed of two of the 4-unit parts previously presented. If, for example, sheets 1, 2, and 3 bore what may be termed parts 1, 2, and 3, then sheets 4, 5, and 6 bore, respectively, parts 1 and 2 combined, parts 1 and 3 combined, and parts 2 and 3 combined. The order of presentation of the parts and of their combinations was either from left to right or from right to left as described above in the pure part method. This distribution of practice was the same under both the massed and the spaced conditions. Sheets 7 to 10 inclusive were, as before, identical with those used for the whole method.

Comparison of the three practice methods will show that when *S* reached the seventh sheet, which in all cases bore the entire key, he had, regardless of his previous practice method, worked the same amount of time, namely, 12 min., on 6 sheets, even though the amount of practice, the total number of substitutions actually performed, might have been different.

Selection of groups. The entire experiment was conducted with college classes of 10 to 40 individuals. The sheets for the three practice methods were distributed to the students as they sat in their seats; one student received the sheets for the whole method, the student sitting next to him those for the pure part method, and the one next to him those for the combination part method, and so on. Hence the original 'selection' was made by chance. When the data were tabulated, however, it was found that this method had given unsatisfactory results; certain individuals were obviously far superior to others in substitution test work and by chance more of these individuals were present in some practice groups than in others. Hence it was necessary to *equate* at least roughly the various groups. This was done by eliminating the scores of individual *Ss* until the following average scores had been rendered approximately equal: (a) those for the whole method massed and the whole method spaced groups on experimental sheet 1;[11] (b) those for the two part method massed and also for the two part method spaced groups on experimental sheet 1; and (c) those for all six groups on the preliminary exercise. In addition all *Ss* who failed for any reason to work the full 2 min. on any sheet were eliminated. Hence the number of *Ss* (281) whose records were used in this study, was considerably less than the number of *Ss* (330), who worked in the experiment.

Results

Tables I and II give, for all three practice groups and for both the massed and the spaced practice, the average number of substitutions made on the preliminary and on every one of the 10 practice sheets, and the accumulative totals of the previous number of substitutions for sheets 2 to 10 inclusive.

Since the time spent in practice was equal for all the groups, the number of substitutions made is the principal criterion available for judging the efficiency of the three practice methods. Furthermore, the number made on sheet 7, which involved for all groups the use of the entire key, is, we presume, the best test of their relative efficiency, since after this trial the part method groups became rather groups whose practice had been with the whole method also.

The massed practice. Inspection of Table I shows the average number of substitutions on sheet 7 to have been for the whole method group 93.8, for the combination part 93.2 and for the pure part 88.8. Hence although the pure part method yielded slightly inferior results there is no very significant difference between the

[11]The results of this procedure can be seen in the tables following which show these scores to have finally been averaged at 71.2 and 71.6.

three groups. The scores on later sheets suggest, however, that the whole method group is slightly inferior and that the part method groups are approximately equal.

TABLE I

SHOWING FOR THE MASSED PRACTICE THE AVERAGE NUMBER OF SUBSTITUTIONS MADE BY THE THREE DIFFERENT PRACTICE GROUPS TOGETHER WITH THEIR ACCUMULATIVE TOTALS

	Whole Method N = 45		Combination Part Method N = 53		Pure Part Method N = 46	
Sheet	No. of subs.	Previous total	No. of subs.	Previous total	No. of subs.	Previous total
P	58.0		58.0		58.4	
1	71.2	0	93.0	0	92.7	0
2	79.3	71.2	90.7	93.0	107.2	92.7
3	83.7	150.5	94.8	183.7	95.3	199.9
4	86.5	234.2	84.9	278.5	115.5	295.2
5	89.5	320.7	97.7	363.4	101.0	410.7
6	91.9	410.2	95.5	461.1	118.7	511.7
7	93.8	502.1	93.2	556.6	88.8	630.4
8	94.9	595.9	99.7	649.8	100.0	719.2
9	98.1	690.8	102.8	749.5	101.9	819.2
10	100.1	788.9	106.1	852.3	106.7	921.1

TABLE II

SHOWING FOR THE SPACED PRACTICE THE AVERAGE NUMBER OF SUBSTITUTIONS MADE BY THE THREE DIFFERENT PRACTICE GROUPS TOGETHER WITH THEIR ACCUMULATIVE TOTALS

	Whole Method N = 43		Combination Part Method N = 47		Pure Part Method N = 47	
Sheet	No. of subs.	Previous total	No. of subs.	Previous total	No. of subs.	Previous total
P	57.3		56.6		57.7	
1	71.6	0	93.1	0	92.6	0
2	78.7	71.6	95.1	93.1	95.8	92.6
3	86.4	150.3	97.1	188.2	98.5	188.4
4	91.9	236.7	88.9	285.3	112.3	286.9
5	98.9	328.6	95.7	374.2	113.2	399.2
6	101.8	427.5	98.6	469.9	112.5	512.4
7	108.5	529.3	102.0	568.5	96.1	624.9
8	113.7	637.8	110.7	670.5	107.0	721.0
9	116.4	751.5	112.0	781.2	111.2	828.0
10	118.0	867.9	112.8	893.2	115.1	939.2

It is worth noting, however, that the part method groups had more practice in the sense of actually performing more substitutions than had the whole method group. For example, although the latter made on sheet 10 a score of 100.1 only, compared with scores of 106.1 and 106.7 for the part method groups, they had previously made only 788.9 substitutions, compared with 852.3 and 921.1 for the other two groups. There was, however, no way

in this experiment of equalizing both time and amount of practice, and of the two time is presumably the more important criterion. Furthermore, inspection of the various accumulative totals shows that even if time were disregarded and amount of practice taken as the criterion the whole method group would still fail to become superior. To illustrate: the pure part group previous to work on sheet 8, on which they scored 100.0, had made 719.2 substitutions. The whole method group had not made as many as 719 until they reached sheet 10, previous to which they had made 788.9. But even on this sheet they scored only 100.1, no higher than the pure part group had on sheet 8.

The spaced practice. Inspection of Table II shows that according to the criterion of number of substitutions on sheet 7 the whole method was superior, the pure part method inferior, the combination part method intermediate in efficiency. Furthermore, although on later trials the pure part comes to equal or even slightly surpass the combination part group, the whole method group remains superior throughout. It may also be noted that the amount of practice, *i.e.* the total number of substitutions previously made, was less for every sheet for the whole method group.

Massed vs. spaced. Comparison of the scores made on sheets 7 to 10 inclusive under massed and spaced conditions shows: (1) The scores of all three practice groups are without exception higher in the spaced practice on all of the above 4 trials. (2) The whole method group is in the spaced practice consistently superior to both the part method groups, whereas in the massed it is at no time superior to the combination part and is superior to the pure part on sheet 7 only. Hence the spacing was of greater advantage to the whole than it was to either part method; or, conversely, massing the practice was most detrimental to the whole method.[12]

[12]The reliabilities of these differences in the scores on sheet 7 are:

		Diff.	S.D.
Massed practice,	Whole-Combination part	0.6	3.52
	Whole-Pure part	5.0	3.73
	Combination part-Pure part	4.4	4.06
Spaced practice,	Whole-Combination part	6.5	5.19
	Whole-Pure part	12.4	5.28
	Combination part-Pure part	5.9	5.46
Massed-Spaced,	Whole method	14.7	4.17
	Combination part method	8.8	4.68
	Pure part method	7.3	4.94

Discussion

The results of this study show that in the massed practice the whole method is consistently inferior (though sometimes but slightly) to both of the part methods used. This inferiority may be due to the following factors.

(1) To the absence of relationships. The absence of important relations (certainly of the serial and spatial) between the reactions learned in this study lends support to the view that the value of the whole method is correlated with the degree of relationship between the responses acquired.

(2) To the effects of transfer. As Pechstein pointed out,[13] and as our results clearly show,[14] positive transfer from one part to another, and the "avoidance of diminishing returns" are factors that intrinsically favor a part method.

(3) To emotional factors. We must consider also, in the interpretation of our results, the factors that Woodworth calls "the emotional," *i.e.* "interest, confidence, and visible accomplishment,"[15] which are closely related to what Pechstein characterizes as the less emotional disturbance, the greater "consciousness of power."[16] It seems probably that in our work with the substitution test the part method groups were, as compared with the whole method group, favorably stimulated by their greater "visible accomplishment" (as evinced by their higher scores on the earlier sheets) and that this factor, together perhaps with the constantly changing, relatively non-monotonous character of their work, helped to maintain in them a greater confidence, interest and incentive—in short a higher degree of motivation.

(4) To fatigue. We must also, following Pechstein further,[17] point to the relation between the difficulty of a given task or problem and the likelihood that the whole method under massed conditions will prove the most economical. A whole method is presumably a more fatiguing, a more effortful mode of practice

[13] Pechstein, *Psychol. Monog., op. cit.*, 58.

[14] Shown most clearly in the present experiment by the higher scores made by the pure part group under the massed condition on part 2 (practised in sheets 3 and 4) than in part 1 (practised in sheets 1 and 2), and by the still higher scores on part 3 (practised in sheets 5 and 6).

[15] Woodworth, *op. cit.*, 345.

[16] Pechstein, *ibid.*, 57.

[17] Pechstein, Massed *vs.* distributed effort in learning, *J. Educ. Psychol.*, 12, 1921, 92-97.

than a part method—for one reason perhaps because of the emotional factors mentioned above—and massed practice is presumably more fatiguing than spaced. Hence for a hard problem the combination of whole method and unspaced practice may constitute a highly ineffective mode of learning. In the present case it is doubtful whether a simple 12-unit substitution test can be classified as a 'hard problem,' but observation showed that for the *S*s of all three practice groups the continuous work with the 10 sheets was a hard task in the sense at least of being a fatiguing one. The factor of fatigue may, therefore, be of especial significance because it would be apt (since it tends to lessen interest and to be itself augmented as incentive decreases) to affect most visibly the mode of practice which is likely to be the least motivated; namely, the whole method.

The results of this study show, in confirmation of the results obtained by Pyle,[18] by Dearborn, [19] and by Starch,[20] that spaced practice is more efficient than unspaced.

We also find, in confirmation of Pechstein's results with the maze, that the whole method is more efficient under spaced than under massed practice. This result suggests that the relative inferiority of the whole method, when the practice was massed, may have been due less to factors intrinsic to it as a mode of learning than to the effect of such relatively extrinsic influences as the motivation and fatigue factors previously described. For all such factors as the degree of relationship between the reactions learned, the positive transfer from one part to another, the avoidance of diminishing returns, the greater visible accomplishment, are intrinsic either to the material employed or to the nature of part methods. Hence they would in no way be eliminated, probably not even significantly altered, merely by a spacing of practice periods. One need not, to be sure, assume that through spacing alone learning by the whole method becomes forthwith more interesting or more stimulating, but it may become less fatiguing; and, as we have already pointed out, the effect of fatigue might, so far as scores are concerned, be revealed most clearly with the least motivated method.

[18]W. H. Pyle, Economical learning, *ibid.*, 4, 1913, 148-158.
[19]W. F. Dearborn, Experiments in learning, *ibid.*, 1, 1910, 373-388.
[20]D. Starch, Periods of work in learning, *ibid.*, 3, 1912, 209-213.

Our results, however, disagree with those of Pechstein[21] in two respects. (a) He found the efficiency of both pure and progressive part methods for learning mazes to be much less under spaced than under massed conditions. Our results for substitution tests show both pure and combination part methods to have yielded higher scores with spaced practice. In his explanation, however, he attributes the inferiority of part methods under spaced conditions to their being under such circumstances too easy; the organism does not develop the habits of "long application" which are essential for the final and difficult task of connecting the parts. But when, as in work with a substitution test, the reactions have no such important serial and spatial relationships as they possess in maze learning, there is of course no difficult connecting of parts to be accomplished. Hence the discrepancy between his results and ours does not suggest that his conclusions are erroneous but only that they are perhaps not applicable to unrelated material.

(b) His most efficient method of practice was the progressive part method massed, our most efficient method was the whole method spaced. Such a disagreement is probably incapable, however, of any completely adequate explanation. The actual score achieved by any method in any given experiment is determined by a great number and variety of factors: determined not only by the intrinsic characteristics of the methods themselves and their adaptability to the particular material employed, but also by such variables as the nature of the always somewhat arbitrarily chosen parts, the nature of the spacing, if any, the degree of motivation aroused and of fatigue engendered, the previous practice habits of the *S*s, and possibly their age and intelligence as well. It is admittedly possible that in the present experiment some one of these factors could have been so varied as to render some form of a part method superior. Nevertheless our data do suggest that for this work with substitution tests the whole method was in some sense really superior to the part methods employed and that spacing the practice had the effect simply of permitting an intrinsic advantage to disclose itself. The source of any such "intrinsic" superiority remains, we admit, uncertain. But it is possible that even in working with a substitution test the *S*s do relate the elements thereof in a manner advantageous to rapid learning; that, for example, they find it

[21] Pechstein, *loc. cit.*

easier to learn that 'b' is 10 if they also observe that 'e' is 9, 'g' 11, 'd' 1, etc. If this be true, then a learning-method which permitted from the first observation of and work with the entire key might finally reveal itself as the most efficient.

Summary

The purpose of the experiment was to compare the efficiency of whole, pure part, and combination part methods for learning material unrelated either serially or spatially, under both massed and spaced conditions of practice. The material selected was a simple letter-number substitution test. The total amount of practice, 20 min., was under the massed condition approximately continuous, but under the spaced was distributed over 3 days separated by 48-hr. intervals. Under the massed conditions the whole method proved to be slightly inferior, or at most equal, to the part methods. This result was thought to support the general opinion that the efficiency of a whole method is correlated with the degree of relationship between the reactions learned. Under the spaced condition, however, the whole method was consistently superior to either part method, and was moreover more efficient than any mode of learning had been under the massed condition. These results agree with those of Pechstein[22] for mazes respecting the greater efficiency of the whole method when practice is spaced. They differ from his, however, in finding the part methods also more effective when spaced and in finding the whole method spaced to be the most economical of all. Although various suggestions were advanced the explanation of these differences remained uncertain. But in any case it seems unlikely that the relative values of whole and part methods in maze learning, which demands the acquisition of responses closely related both serially and spatially, would necessarily be identical with their values for acquiring the unrelated reactions involved in work with substitution tests.

[22]Pechstein, *loc. cit.*

THE EFFECT OF DEHYDRATION ON PAROTID SECRETION[1]

By A. L. Winsor, Cornell University

In a recent extended investigation of factors affecting the occurrence of seizures in epileptic children, McQuarrie has indicated what appears to be a significant relationship between water balance, or the state of dehydration of the body, and the frequency of seizures.[2] Children having as many as twenty seizures a day when the intake of water approximated 2,000 cc. had no attacks when the water intake was reduced to 200 cc. per day. Although a special diet was provided throughout the experiments, McQuarrie concluded that the water balance was the significant factor. The following statements appear in his conclusions: "Convulsions tend to occur when a positive water balance above a certain magnitude is established." . . . "Suddenly increasing the intake of water during the course of treatment by dehydration tends to cause recurrence of seizures at least in the severe cases." "A disturbance in water balance, perhaps affecting the central nervous system more specifically, appears to be closely identified with the etiology of epilepsy."[3]

This study suggests the possibility of water exchange playing a more important rôle in mental phenomena than has been suspected. Although extensive research has been carried on to determine the physiological and psychological effects of deprivation of food or oxygen, very little attention has been given to the study of the influence of the reduction of water intake. In general it has been assumed and recommended that each individual should flood his system with liquids each day. While such a dictum might have value in some physiological processes, it is possible that it might have disadvantages for at least some individuals. It seems desirable, therefore, that investigations of the effect of water balance on mental activity be undertaken.

Some of the obvious difficulties which confront such an investigation are lack of practical technique for determination of the state of dehydration of an individual at any given time, and lack of exact information on the relative value of various methods of dehydration. The common method employed by physicians has been to determine the quantity and quality of urine excreted over a given period while controlling the intake of liquids. This method has its limitations and the technique and service essential for such a determination is not always available to the person primarily interested in mental phenomena. It is the purpose of this report to suggest another method whereby the state of dehydration may be determined.

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[1]The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research at Cornell University.

[2]Irvine McQuarrie, Epilepsy in children: the relationship of water balance to the occurrence of seizures, *Amer. J. Diseases Children.* 38, 1929, 1-17.

[3]*Op. cit.*, 16 f.

Early in his study of gastric secretions in dogs Pavlov noted that these juices were definitely diminished when the intake of fluids was reduced.[4] More recently Gantt,[5] also in the Russian laboratories, found that a reduction of water intake influenced both conditioned and unconditioned salivary reflexes in children. The secretion from both the submaxillary and parotid glands was observed in his study. Cannon,[6] apparently collecting the total salivary secretions at intervals over a period of 20 hours, noted in his own case that reduced liquid consumption lowered by 50% the amount of secretion evoked by chewing tasteless rubber. Similar observations have been made by other investigators working with rabbits and dogs.

Since the quantity of parotid, submaxillary, and gastric secretions and probably of other digestive juices varies in accordance with the amount of liquid in the organism, a quantitative analysis of the secretion of any or all of these glands should afford some index of the water balance of the body. Because of the fact that the digestive secretions are composed largely of water any significant limitation of liquid intake would of course affect them first.

Method

In this series of experiments we have attempted to determine more exactly the extent to which the volume of digestive secretions may be used as an indication of water balance and to evaluate the dehydrating influence of various agencies. Because of their accessibility the parotid glands were used for the investigation. A suction cup was applied over the mouth of Stenson's duct, and the secretion was collected in a graduated tube. A stop-watch was used to time the intervals and readings were made at the end of every minute. Adult male *Ss* were used for the tests.

In order to determine the most consistent and reliable type of stimulation to be used in estimating water balance, an experiment was arranged whereby several sources of stimulation could be used and their relative merits evaluated. By the first method the flow was measured for fifteen minutes while the *S* chewed tasteless gum between the molars on the side from which the secretion was being drained. A metronome beating 60 times per min. provided a timing device to keep uniform the number of chews for each minute and the *S* was instructed to keep his lips closed while he maintained an ordinary chewing pressure. He swallowed on signal at the end of each minute. Following the chewing activity the *S* removed the gum and sat quietly awaiting the next experiment.

The second method involved no direct oral stimulation but attempted to determine the effect of dehydration on the normal flow of saliva. Although the existence of a constant flow of secretion has been overlooked by some investigators our studies[7] support Lashley's[8] findings that such a secretion obtains.

[4] I. P. Pavlov, *The Work of the Digestive Glands*, 1902, 227.

[5] W. H. Gantt, Salivary secretion and the intake of fluid, *Amer. J. Diseases Children*, 37, 1929, 1125-1127.

[6] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1929, 322.

[7] A. L. Winsor, Factors indirectly affecting parotid secretion, *J. Exper. Psychol.*, (in press).

[8] K. S. Lashley, Reflex secretion of the human parotid gland, *ibid.*, 1, 1916, 461-493.

After a 5-min. interval provided for the effect of the chewing to subside, recording of the saliva was resumed for 15 min. to determine the effect of dehydration on this constant although meager flow.

The other method of stimulation was to permit the mucosa of the mouth and throat to become dry and parched, for it had been shown in preliminary tests that this process excited the glands to secrete an extra supply. Cotton was placed in the nostrils and the *S* told to breathe through his mouth by parting his lips slightly without stretching the muscles of the jaw. Swallowing and tongue movements were inhibited throughout the entire thirty minutes of this test. During this period secretion from the other glands of the oral cavity was permitted to flow out of the mouth into an open container over which the subject held his head. The collecting and measuring of the secretion for the various tests consumed about 1.5 hr. and was repeated three or four times a day. Acids were not used for the purpose of exciting secretion because of the difficulties involved in maintaining amounts of equal intensity in a definite area of the mouth and because of the desire to avoid ingesting liquids of any sort during the experiment. After some facility in this technique had been acquired a prolonged thirst was arranged to determine the relative effectiveness of the three situations for indicating the progress of dehydration when the intake of liquids over an extended period was greatly reduced.

Food during the experiment consisted of three pieces of dry toast at each meal. There were three such meals each day at 7:30 A.M., 12:30 P.M., and 6 P.M. Such a diet was designed to reduce the liquids ingested to a minimum and at the same time reduce hunger as a disturbing factor. Throughout the period of the test the subject chewed the same piece of gum so that the mucosa of the mouth and throat would not be dry. Strenuous exercise involving perspiration was avoided but the ordinary activities of the day were carried on as usual.

Results

Figure I shows the quantity of parotid secretion in terms of cubic centimeters obtained for successive trials by the three methods of stimulation. The subject drank his last liquid at 7 P.M. and the numbers on the abscissa represent the successive hours of the thirst after that time. Curve A represents the amount of secretions from one parotid gland when *S* chewed tasteless gum for 15 min. Curve B represents the secretion resulting from 30 min. of mouth breathing, and curve C represents the normal secretion for 15 min.

It will be observed that the secretion from the chewing activity showed a consistent reduction after the first day with significant diurnal changes in the early stages of the thirst. At the end of the thirst the secretion had been reduced to about one-sixth of its normal amount but when 1000 cc. of water were consumed during a 5-min. interval it was restored within a very few minutes to 11 cc. The *S* began to perspire as soon as he drank the water. His temperature had been reduced to 96.6°F.

The lower curves show the same general characteristics as Curve A except that their variations are less marked. Since the demands here on the liquid supply of the body are moderate compared to the chewing capacity it would be expected that mild dehydration would have but a slight effect on the normal

flow. It would appear that some activity such as uniform chewing on some tasteless substance might provide the most satisfactory technique for determining the stage of dehydration of an individual with this method.

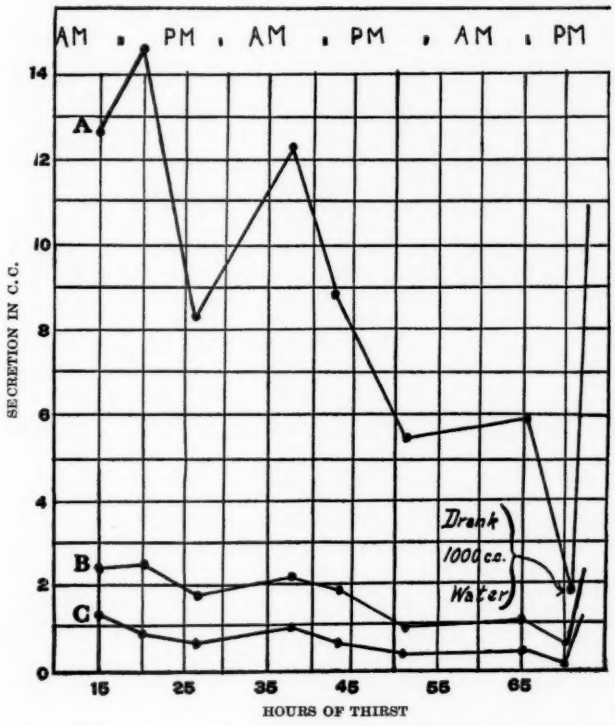

FIG. 1. CURVES SHOWING THE INFLUENCE OF DEHYDRATION ON PAROTID SECRETION FROM ONE GLAND OF AN ADULT SUBJECT

(A represents the volume secreted for successive periods when tasteless gum was chewed for 15 min.; B shows the amount for the same periods when *S* breathed through his mouth for 30 min.; and C represents the normal flow for 15 min.)

The viscosity of the secretion collected was determined with a viscosity tube after each test but no significant changes occurred. Although the small amount of secretion in the mouth on the last day appeared to be very viscous that collected from the disk showed no change. A serous gland, such as the

parotid, apparently continues to secrete its characteristic thin watery fluid even in thirst, while the mucous glands deliver a secretion which becomes more viscid with dehydration.

Throughout the investigation observations were made on the sensations of thirst. Since it is generally agreed that thirst is an experience referred to the mucous lining of the mouth and pharynx, and especially to the roof of the tongue and palate, we were interested to know what the effect would be if these regions were kept moist by constant chewing while the water balance of the organism was greatly reduced. It will be recalled that the *S* chewed gum constantly except when asleep or undergoing tests where chewing was not a part of the procedure. As long, therefore, as chewing evoked secretion the entire oral cavity would be kept moist. 'False thirst,' caused from a drying of the mucosa, was thus eliminated, except when the *S* was breathing through his mouth or when he first awoke. 'True thirst,' on the other hand, which represents an actual bodily need as a result of insufficient liquid in the organism, expressed itself after the second day in the form of a general craving for liquids. This craving was dominant over other desires, and seemed much like the craving for sweets soldiers experienced after long periods of deprivation at the front, or it might be similar to the craving men sometimes have for salt when living on a salt-free diet for an extended period. It did not seem to be assignable to any particular region of the organism as in 'false thirst.' The eating of toast became more or less of a task before the experiment was concluded so completely had other desires been submerged by that for liquids.

During some previous tests that were made in which gum was not chewed and the mouth was not kept moist, discomfort arose from the drying of the tongue and palate as soon as the water supply of the organism began to get low. It seems clear that when liquids are withheld from the body, the membranes around the base of the tongue are affected first and that dryness of these tissues is a normal stimulant for salivary secretion. This sensation of dryness is what is ordinarily thought of as thirst, but when this area is not allowed to become dry as in the case of the experiment under discussion, there is still craving for liquids which would provide protection against the body's becoming dangerously dry. 'False thirst' may or may not be a reliable index of dehydration. A few minutes of mouth breathing will cause one to want to drink although the organism may be saturated with fluid. 'True thirst,' on the other hand, is perhaps always an indication of a low water balance.

Since this study was primarily concerned with technique for estimating the normal processes of dehydration no effort was made at this time to determine the mental effect of a low water exchange. The *S*'s behavior certainly showed no ill effects and he reported that he felt unusually well and capable of sustained mental effort. A further study is being planned in which the effect of dehydration on various phases of behavior will be studied. At the present time it seems probable that at least some individuals reduce their efficiency and perhaps health by overloading their bodies with liquids.

Incidentally the influence of various stages of dehydration on psychogalvanic responses needs to be taken into consideration by those studying behavior with this technique. Obviously the conductivity of the organism would change as the water balance changed.

A Rapid Method of Dehydration

While the normal process of dehydration shown in Chart I proceeded over a period of several days, it is possible to speed up the process materially. It is a common observation that strenuous bodily activity or the subjection of the body to heat causes a rapid lowering of the water balance as indicated both by the consumption and excretion of water. Some idea of the rate at which this process takes place may be gained from an experiment with hot water. The *S* chewed the tasteless gum for 15 min. as in the previous experiment and then reclined in a tub of water the temperature of which remained between 96° and 100°F for one hour. Throughout this process he perspired profusely. At the end of the hour the same chewing activity evoked approximately 50% of the secretion that was obtained before the hot bath was taken. A state of dehydration was apparently reached in one hour by this method which had required from 24–48 hr. by simply stopping the intake of liquids. If a low water exchange is desired in the treatment of abnormal behavior it may be obtained in a short time by this procedure. The known value of hydrotherapy with certain types of mental disturbance may be at least partially due to a reduced water balance. Here again the ingestion of a pint of water restored the organism to its original state.

Aspects of glandular activity which need to be observed in determining the stage of dehydration such as diurnal variations, daily variations, differences in glands on opposite sides of the mouth, and the influence of rest periods on glandular functioning will be considered in a subsequent report.

Summary

The results of this study may be summarized as follows:

(1) The beneficial effect of dehydration in preventing epileptic seizures in some cases studied suggested the need for an investigation of the influence of a low water balance on mental behavior.

(2) A method for determining the state of liquid exchange through a quantitative analysis of parotid secretion under different exciting situations was presented, and the normal progress of dehydration when the intake of liquid was materially reduced over a period of 70 hrs. was charted.

(3) Sensations referred to the mucous lining of the mouth and pharynx called 'false thirst' were prevented during the experiment by the constant chewing of tasteless gum. 'True thirst,' however, expressed itself when the actual bodily need for liquid arose in the form of a dominant craving for liquids. 'True thirst' is apparently a much more reliable index of low water balance than 'false thirst.'

(4) A hot bath causing excessive perspiration was found to reduce the secretions approximately fifty percent in one hour. The water balance was quickly restored when liquid was ingested.

SOME EFFECTS OF HETEROGENEITY ON THE THEORY OF FACTORS

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The effects of heterogeneity in the experimental group upon the results of investigations designed to measure the extent and relative importance of mental abilities have been the subject of considerable controversy. Spearman suggests that such heterogeneity may give rise to spurious group factors which may tend to obscure the general factor.[1] Kelley questions this statement, and maintains that heterogeneity should spuriously increase the relative importance of the general factor.[2] He lists as the principal types of such heterogeneity differences in maturity, race, sex, and general nurture.

In the spring of 1929, the American Council Psychological Test, Series of 1928, was given to all applicants for admission to the University of Hawaii and the Territorial Normal and Training School. A considerable portion of this material was made available to the writers.[3] At the largest high school in Honolulu, the tests were given to all seniors, regardless of application or non-application, as a regular part of the guidance program. At all other schools, the testing was carried on directly by the University and Normal School, and this latter group of test papers, numbering 396, constitutes the experimental data of this paper.

Since the group tested consisted entirely of high school seniors, we may assume that there are no important differences in maturity; *i.e.* that, with reference to the test abilities, the subjects may all be considered adult, or practically so. We may also assume that one, and probably the most important single one, element in general nurture, namely, scholastic background—is constant. The two sexes are represented in about equal numbers. In regard to race, the heterogeneity is extreme. The group contains considerable numbers of Chinese, Haoles,[4] Hawaiians, Japanese, Koreans, and Portuguese, together with a sprinkling of other races, and a fairly large number of students of mixed ancestry.

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[1]C. Spearman, *The Abilities of Man*, 1927, 155-7. (Hereafter referred to as *Abilities*.)

[2]T. L. Kelley, *Crossroads in the Mind of Man*, 1928, 17-19. (Hereafter eferred to as *Crossroads*.)

[3]The writers are indebted to Dr. E. V. Sayers, Director of Education of the Territorial Normal and Training School, and Professor T. M. Livesay, Head of the Department of Education of the University of Hawaii, for permission to use this valuable material.

[4]North Europeans and American whites.

The test consists of five sub-tests. The first of these is a completion test. In each sentence, only one word has been omitted, and the number of letters in the omitted word is given, so that of the otherwise possible responses only one will be correct. There are 40 sentences. The second is an artificial language test. A vocabulary of 10 words, and 6 rules of grammar are given. Following are 20 sentences to be translated. Alternate sentences are in English and the artificial language. Every word correctly translated counts one point, provided the subject does not skip an entire sentence, in which case no further credit is given. There are 73 words altogether. The third is an analogies test of spatial relations. At the left of each row are three geometric figures, and at the right, five. The subject must pick from the five the one bearing the same relation to the third that the second does to the first. There are 28 items in this test. The fourth consists of 20 arithmetic reasoning problems. The last is an opposites test. On each line are four words, two of which are either synonyms or antonyms. The subject must pick these and indicate whether they are the same or the opposite.

All of these tests seem to satisfy Spearman's demand that they call for eduction rather than reproduction on the part of the subject; and all are sufficiently dissimilar so that any group factors found cannot be ascribed to overlap 'due to conspicuous resemblance between tests'.[5] They therefore would seem to be well suited for an investigation of the effects of heterogeneity upon the relative strengths of general, group, and specific factors.

In this study, an analysis has been made of the factors in the total group of 396, and in a sub-group of 87 boys of pure Japanese ancestry, in an attempt to determine the effects of race and sex heterogeneity. In order to carry out this investigation it was necessary to devise certain statistical tools.

Statistical Theory

The basic technique in the investigation of the relative importance of group factors is the tetrad, which may be defined by the equation,

$$t_{1234} = r_{12}r_{34} - r_{13}r_{24},$$

the first two subscripts of the t determining the first r, and the first and third subscripts, the third r, so that

$$t_{1243} = r_{12}r_{34} - r_{14}r_{23}, \text{ and}$$
$$t_{1342} = r_{13}r_{24} - r_{14}r_{23}.$$

The standard error of the tetrad as given by Kelley is

$$\sigma_{t_{1234}} = 1/\sqrt{N}\,[r^2_{12} + r^2_{13} + r^2_{24} + r^2_{34} + 2r_{12}r_{14}r_{23}r_{34} + 2r_{13}r_{14}r_{23}r_{24} - 2r_{12}r_{13}r_{23} - 2r_{12}r_{14}r_{24} - 2r_{13}r_{14}r_{34} - 2r_{23}r_{24}r_{34} + t^2_{1234}(r^2_{12} + r^2_{13} + r^2_{14} + r^2_{23} + r^2_{24} + r^2_{34} - 4)]^{\frac{1}{2}} \ldots [1]$$

As an approximation to the standard error of any random tetrad in a table of all the tetrads of a given system, the writers propose

$$\sigma_{t_4} = 2/\sqrt{N}\,[r'^2(1-r')^2 + s^2 - (\Sigma t^2_4/3_nC_4)(1 - 1.5R^2)]^{\frac{1}{2}} \ldots\ldots\ldots\ldots [2]$$

[5] *Abilities*, 150-153.

where t_4 is the random tetrad;[6]

N the total frequency;

n the number of variables;

$_{3n}C_4$ the number of tetrads;

r′ the average intercorrelation between variables;

R^2 the average of the squares of the intercorrelations; and

s^2 the mean square variation of the intercorrelations from their mean, so that $s^2 = R^2 - r'^2$.

Formula [2] was arrived at by substituting for each of the intercorrelations in [1] the average intercorrelation r′; for each of the squared intercorrelations, the mean square intercorrelation R^2; and for t^2_{1234}, the mean square tetrad.

The remainder of this section will be devoted to a consideration of methods for investigating the relative importance of specific factors. The basic function in this analysis will be designated as the triad.

As a preliminary, we shall note certain theorems.

Proposition I. Two variables may be thought of as due to one general factor and no specific factors when their correlation, corrected for attenuation, is equal to one. The errors of measurement always introduce chance specific factors whose magnitudes depend upon the reliabilities of the tests used rather than upon the mental make-up of the *Ss* and such errors should be allowed for by using correlations corrected for attenuation, except in formulas such as the tetrad, in which they cancel.

Proposition II. The variance of the specific factor in either of two correlated variables, expressed as a fraction of the total non-chance variance of such variable, lies between the values $1 - r_{\infty\omega}$ and $1 - r^2_{\infty\omega}$. By a specific factor, we mean any part of the variance contributed by factors other than those which are common to the two variables under consideration. Any factor is specific only with regard to a specified system. This proposition has been proved by R. C. Tryon.[7] Proposition I is a special case of II.

Proposition III. Three variables may be thought of as due to a single general factor and two specific factors when the product of two of the intercorrelations, corrected for attenuation, is equal to the third. We define

$$\alpha_1 = c_1\sigma_g/\sigma_\infty, \quad \alpha_2 = c_2\sigma_g/\sigma_\omega, \quad \alpha_3 = c_3\sigma_g/\sigma_\gamma$$

in the system

$$x_\infty = c_1 g, \quad x_\omega = c_2 g + s_2, \quad x_\gamma = c_3 g + s_3$$

where x_∞, x_ω and x_γ are the variables x_1, x_2 and x_3, corrected for attenuation;

g is the general factor;

s_2 and s_3 are factors specific to x_2 and x_3 respectively;

c_1, c_2, and c_3 are constant weights.

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[6]The single subscript 4 after t indicates a tetrad. The single subscript 3 after t will designate a triad. The triad will be defined later.

[7]R. C. Tryon, Interpretation of correlation coefficients, *Psychol. Rev.*, 36, 1929, 419-445.

In this system,

$$\alpha^2_1 = 1$$
$$\alpha_1\alpha_2 = r_{\infty\omega}$$
$$\alpha_1\alpha_3 = r_{\infty\gamma}$$
$$\alpha_2\alpha_3 = r_{\omega\gamma}$$

whence

$$r_{\infty\omega}r_{\infty\gamma} = r_{\omega\gamma}$$

and

$$t_{123} = r_{\infty\omega}r_{\infty\gamma}/r_{\omega\gamma} = 1 \ldots\ldots\ldots\ldots\ldots [3]$$

This equation is the triad. It is similar to Kelley's equation [14][8] except that all correlations are corrected for attenuation. The variable, x_∞, which in this system has no specific factor, appears twice in the numerator of the triad, but not in the denominator. If x_ω were the variable without any specific factor, the triad would become

$$t_{231} = r_{\infty\omega}r_{\omega\gamma}/r_{\infty\gamma} = 1$$

and if x_γ were the variable without a specific factor, it would be

$$t_{312} = r_{\infty\gamma}r_{\omega\gamma}/r_{\infty\omega} = 1.$$

As a corollary, we have:

Proposition IV. n *variables may be thought of as due to one general factor and* n-1 *specific factors when all the triads which involve one variable in the numerator twice are equal to one.* Kelley's Proposition 11 is a special case of the above.[9] His equations [29] will be shown to be equivalent to our triads.

Let r_{12}, r_{13} and r_{23} be intercorrelations between variables;
r_1, r_2 and r_3, reliabilities for the respective variables, obtained by correlating random halves;
R_1, R_2 and R_3, estimated reliabilities for the total variables, according to the equations

$$R_1 = 2r_1/(1 + r_1),\ R_2 = 2r_2/(1 + r_2),\ R_3 = 2r_3/(1 + r_3).$$

Then $r_{\infty\omega} = r_{12}/\sqrt{R_1R_2},\ r_{\infty\gamma} = r_{13}/\sqrt{R_1R_3},\ r_{\omega\gamma} = r_{23}/\sqrt{R_2R_3}, \ldots\ldots [4]$

and $$t_{123} = r_{\infty\omega}r_{\infty\gamma}/r_{\omega\gamma} = r_{12}r_{13}/r_{23}R_1 \ldots\ldots\ldots\ldots\ldots [5]$$

Also $$r_{\infty 2} = r_{12}/\sqrt{R_1},\ r_{\infty 3} = r_{13}/\sqrt{R_1},$$

and $$t_{123} = r_{\infty 2}r_{\infty 3}/r_{23} = r_{12}r_{13}/r_{23}R_1 \ldots\ldots\ldots\ldots\ldots [5a]$$

The second expression of equation [5a] is the first of Kelley's equations [29], and is seen to lead to a value identical with [5]. Similarly

$$t_{231} = r_{\infty\omega}r_{\omega\gamma}/r_{\infty\gamma} = r_{\omega 1}r_{\omega 3}/r_{13} = r_{12}r_{23}/r_{13}R_2,$$
$$t_{312} = r_{\infty\gamma}r_{\omega\gamma}/r_{\infty\omega} = r_{\gamma 1}r_{\gamma 2}/r_{12} = r_{13}r_{23}/r_{12}R_3.$$

Proposition V. If three variables may be thought of as due to one general factor and three specific factors, then the correlation between the general factor and any

[8] *Crossroads*, 40.
[9] *Crossroads*, 50-51.

one of the variables will be equal to the square root of the triad involving that variable twice in the numerator.

Let $x_\infty = c_1 g + s_1$
$x_\omega = c_2 g + s_2$
$x_\gamma = c_3 g + s_3$.

Then $r_{\infty\omega} = \alpha_1\alpha_2$
$r_{\infty\gamma} = \alpha_1\alpha_3$
$r_{\omega\gamma} = \alpha_2\alpha_3$

and $t_{123} = r_{\infty\omega}r_{\infty\gamma}/r_{\omega\gamma} = \alpha^2_1$.

Now $r_{\infty g} = r_{g(g+s_1)} = (c^2_1\Sigma g^2 + c_1\Sigma g s_1)/Nc_1\sigma_g\sigma_{(g+s_1)}$

but by definition r_{gs_1} is zero, so that

$$r_{\infty g} = c_1\sigma_g/\sigma_\infty = \alpha_1 = \sqrt{t_{123}} \quad \ldots\ldots\ldots\ldots\ldots\ldots [6]$$

Spearman, by a different line of proof, has arrived at a similar result,[10] except that he used correlations uncorrected for attenuation, so that his specific factors included the errors of measurement. Proposition III may be seen to be the special case of V in which the correlation between the variable and the general factor is equal to one.

From [6], $t_{123} = c^2_1\sigma^2_g/\sigma^2_\infty$, but since $x_\infty = c_1 g + s_1$, and s_1 is uncorrelated with g,

$$\sigma_\infty{}^2 = c^2_1\sigma^2_g + \sigma^2_{s_1}$$
$$c^2_1\sigma^2_g/\sigma^2_\infty + \sigma^2_{s_1}/\sigma^2_\infty = 1 \quad \ldots\ldots\ldots\ldots\ldots\ldots [7]$$

so that the variance of the variable x_∞ may be considered to be composed of two independent portions, one determined by g and one by s_1, the relative respective contributions being t_{123} and $1 - t_{123}$.

If group factors are present in addition to the general and specific factors, $1 - t_{123}$ should in most cases give a fair approximation to the relative magnitude of the specific factor in x_∞; so that if we have two bodies of data, we may compute all the possible triads for each system; the mean of all the triads in one system compared with the mean of all those in the other should then give some indication of the relative importance of specific factors in the two systems.

In general, before we can use the triad effectively, we need its standard error. The derivation is as follows:

Let x be measured by fallible scores x_1 and x_I whose sum is x_1.

Then $r_{1I} = r_1$ of our previous notation, and $R_1 = 2r_1/(1 + r_1)$, as before.

From [5], $t_{123} = r_{12}r_{13}/r_{23}R_1$.

Taking logarithmic differentials, squaring, summing and dividing by the population,

$$\sigma^2_{t_{123}}/t^2_{123} = \sigma^2_{r_{12}}/r^2_{12} + \sigma^2_{r_{13}}/r^2_{13} + \sigma^2_{r_{23}}/r^2_{23} + \sigma^2 R_1/R^2_1 - (\sigma_{R_1\sigma_{r_{12}}r_{R_1r_{12}}})/R_1r_{12}$$
$$-(\sigma_{R_1\sigma_{r_{13}}r_{R_1r_{13}}})/R_1r_{13} + (\sigma_{R_1\sigma_{r_{23}}r_{R_1r_{23}}})/R_1r_{23} + (\sigma_{r_{12}\sigma_{r_{13}}r_{r_{12}r_{13}}})/r_{12}r_{13}$$
$$-(\sigma_{r_{12}\sigma_{r_{23}}r_{r_{13}r_{23}}})/r_{12}r_{23} - (\sigma_{r_{13}\sigma_{r_{23}}r_{r_{13}r_{23}}})/r_{13}r_{23} \quad \ldots\ldots\ldots\ldots\ldots\ldots [8]$$

[10] *Abilities*, Appendix, XVI.

Assuming normal distributions in the sampled population and a large sample, we may obtain $\sigma_{r_{12}}$, $\sigma_{r_{13}}$ and $\sigma_{r_{23}}$ by the usual formula, $\sigma_r = (1-r^2)/\sqrt{N}$. Under the same assumptions, Shen has shown[11] that $\sigma_R = 2(1-R)/\sqrt{N}$. These formulas enable us to evaluate the first four terms of [8]. The last three may be evaluated by Pearson and Filon's formula.[12] To evaluate the fifth and sixth terms, we require two similar product-moment coefficients. For the first of these, taking differentials, $dR_1 = 2dr_1/(1+r_1)^2$, $dr_{12} = dr_{12}$. Multiplying, summing and dividing by the population,

$$\sigma_{R_1}\sigma_{r_{12}}r_{R_1r_{12}} = 2\sigma_{r_1}\sigma_{r_{12}}r_{r_1r_{12}}/(1+r_1)^2.$$

Assuming that $\sigma_1 = \sigma_I$, $r_{11} = r_{II} = (1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}}$.

Assuming in addition that r_{12} and r_{I2} are equal, $r_{12} = r_{I2} = r_{12}(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}}$.

Substituting these values in Pearson and Filon's formula,[13]

$$r_{r_1r_{12}} = r_{12}/2, \text{ so that}$$
$$\sigma_{R_1}\sigma_{r_{12}}r_{R_1r_{12}} = (r_{12}/N)(1-r^2_{12})(1-R_1)$$

and similarly,

$$\sigma_{R_1}\sigma_{r_{13}}r_{R_1r_{13}} = (r_{13}/N)(1-r^2_{13})(1-R_1).$$

To evaluate the seventh term of [8], we require one further product-moment coefficient. Proceeding as before,

$$dR_1 = 2dr_1/(1+r_1)^2,\ dr_{23} = dr_{23}, \text{ and}$$
$$\sigma_{R_1}\sigma_{r_{23}}r_{R_1r_{23}} = 2\sigma_{r_1}\sigma_{r_{23}}r_{r_1r_{23}}/(1+r_1)^2.$$

Assuming as before that $\sigma_1 = \sigma_I$,

$$r_{12} = r_{I2} = r_{12}(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}},\ r_{13} = r_{I3} = r_{13}(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}},$$

and from Pearson and Filon's formula,

$$\sigma_{r_1}\sigma_{r_{23}}r_{r_1r_{23}} = [(1 + r_1^2/2N)][2r_{12}r_{13} - r_{23}(r^2_{12} + r^2_{13})].$$

Substituting in [8], we have for the standard error of the triad,

$$\begin{aligned}\sigma_{t123} = t_{123}/\sqrt{N}[&(1-r^2_{12})^2/r^2_{12} + (1-r^2_{13})^2/r^2_{13} + (1-r^2_{23})/r^2_{23}\\
&+ 4(1-r_1)^2/(2r_1)^2 - (1-r^2_{12})(1-r_1)/2r_1 - (1-r^2_{13})(1-r_1)/2r_1\\
&+ (1/r_{23})(1-r_1/2r_1)\{2r_{12}r_{13} - r_{23}(r^2_{12}+r^2_{13})\}\\
&+ (1/2r_{12}r_{13})\{(1-r^2_{12}-r^2_{13})(2r_{23}-r_{12}r_{13}) + r_{12}r_{13}r^2_{23}\}\\
&- (1/2r_{12}r_{23})\{(1-r^2_{12}-r^2_{23})(2r_{13}-r_{12}r_{23}) + r_{12}r^2_{13}r_{23}\}\\
&- (1/2r_{13}r_{23})\{(1-r^2_{13}-r^2_{23})(2r_{12}-r_{13}r_{23}) + r^2_{12}r_{13}r_{23}\}]^{\frac{1}{2}} \quad \ldots . [9]\end{aligned}$$

This is the full formula for the standard error of a single triad, under the assumptions above noted.

As an approximation to the standard error of any random triad in a table of all the triads of a given system, the writers propose

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[11]E. Shen, The standard errors of certain estimated coefficients of correlation, *J. Educ. Psychol.*, 15, 1924, 462-465.

[12]T. L. Kelley, *Statistical Method*, 1923, 170, formula [129].

[13]T. L. Kelley, *ibid.*, formula [128].

$$\sigma_{t_3} = (T^2/N)^{\frac{1}{2}} [1.5r'^2 + 3/r'^2 + r' - 1/r' + 1/r'_1{}^2 - (3-r')/r'_1 - 3.5]^{\frac{1}{2}} \ldots [10]$$

where t_3 is a random triad

$T^2 = \Sigma t^2_3 / {}_3C_3$

N is the total frequency;

n the number of variables;

${}_3C_3$ the number of triads;

r' the average intercorrelation of the total scores;

r'_1 the average reliability of the half-scores.

Formula [10] was obtained by substituting for each intercorrelation in [9] the mean intercorrelation r'; for each reliability, the mean reliability coefficient r'_1; and for the triad, the root mean square triad $(T^2)^{\frac{1}{2}}$.

Experimental Evidence

The papers used in this study were originally corrected in terms of the total scores for each sub-test. The writers later had them re-scored so as to get comparable halves on each sub-test. In those few cases where the total score on the two halves of a sub-test did not agree with the original score, the paper was scored a third time. For all sub-tests except Artificial Language, the odd and even items were taken as comparable halves. For the latter, items 1, 4, 5, 8, 9, 12, 13, etc. were taken as one half, and 2, 3, 6, 7, 10, 11, etc., as the other; because the odd and even items on this test were respectively English to Artificial and Artificial to English.

The intercorrelations were computed between the total scores on the sub-tests, using a chart devised by the writers which gives an absolute check on all numerical operations. The same chart was used in computing the reliability coefficients. For the latter, the intraclass correlation coefficients were used.[14] Their values were then substituted in the Spearman-Brown formula to give the estimated reliabilities for the total sub-tests. All the intercorrelations were then corrected for attenuation by formula [4]. Tables I and IA give the basic data for the heterogeneous and homogeneous groups respectively.

The corrected intercorrelations are all noticeably less than unity, the largest in the two tables being the r_{18} of Table I, whose value is approximately 0.867 ± 0.015.[15] It is therefore evident that no two variables can be thought of as due to a single general factor plus errors of measurement.

The tetrads were computed for both groups and are given in Tables II and IIA.

The mean of all the tetrads in Table II is 0.0644, and of those in Table IIA, 0.0361. We assume that the standard error of the mean tetrad of a system is equal to $({}_nC_4)^{-\frac{1}{2}}$ times the standard error of a random tetrad from that system. From formula [2], the standard errors of these random tetrads were estimated as 0.0262 and 0.0468 respectively, giving the values 0.0068 and 0.0121 as the standard errors of the corresponding mean tetrads.

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[14] E. E. Cureton and J. W. Dunlap, The basic measure of reliability. Accepted for publication by this Journal.

[15] E. E. Cureton and J. W. Dunlap, Spearman's correction for attenuation and its probable error, this Journal, 42, 1930, 235–245.

TABLE I

INTERCORRELATIONS AND RELIABILITIES FOR THE HETEROGENEOUS GROUPS*
(N = 396)

Tests		1	2	3	4	5
Completion	1	*.7417*				
		.8517				
Art. Lang.	2	.4135	*.9086*			
		.4592	*.9521*			
Analogies	3	.4643	.3910	*.7880*		
		.5359	.4268	*.8814*		
Arithmetic	4	.4570	.3534	.4923	*.4908*	
		.6103	.4464	.6462	*.6584*	
Opposites	5	.7454	.4295	.4716	.3901	*.7672*
		.8668	.4724	.5391	.5159	*.8683*

TABLE IA

INTERCORRELATIONS AND RELIABILITIES FOR THE HOMOGENEOUS GROUP*
(N = 87)

Tests		1	2	3	4	5
Completion	1	*.6512*				
		.7888				
Art. Lang.	2	.3173	*.9291*			
		.3640	*.9632*			
Analogies	3	.3119	.2639	*.8217*		
		.3698	.2831	*.9021*		
Arithmetic	4	.2630	.2265	.4975	*.4369*	
		.3797	.2960	.6717	*.6081*	
Opposites	5	.3446	.2907	.3387	.2792	*.6879*
		.4298	.3281	.3950	.3966	*.8151*

*The upper intercorrelation in each cell is uncorrected. The lower intercorrelation in each cell is corrected for attenuation. The upper reliability coefficient in each cell along the long diagonal is the intraclass correlation between comparable halves of the test. The lower reliability coefficient in each diagonal cell is the Spearman-Brown estimate of the reliability of the total test.

TABLE II

TETRADS FOR THE HETEROGENEOUS GROUP
(N = 396)

Variables a	b	c	d	t_{abcd}	t_{abdc}	t_{acdb}
1	2	3	4	.0395	.0249	−.0146
1	2	3	5	−.0044	−.0965	−.0921
1	2	4	5	−.0350	−.1021	−.0671
1	3	4	5	−.0344	−.1859	−.1515
2	3	4	5	−.0142	−.0589	−.0447

TABLE IIA

TETRADS FOR THE HOMOGENEOUS GROUP
(N = 87)

Variables a	b	c	d	t_{abcd}	t_{abdc}	t_{acdb}
1	2	3	4	.0873	.0885	.0012
1	2	3	5	.0168	.0166	−.0002
1	2	4	5	.0101	.0085	−.0016
1	3	4	5	−.0020	−.0843	−.0823
2	3	4	5	−.0030	−.0709	−.0679

As an empirical check on the accuracy of formula [2], the standard errors of the tetrad having the smallest absolute value (t_{1235}), the tetrad having the largest absolute value (t_{1354}), and the tetrad whose absolute value lay nearest to that of the mean (t_{1452}), in Table II, were computed by formula [1].

$$\sigma_{t_{1235}} = .0177$$
$$\sigma_{t_{1354}} = .0292$$
$$\sigma_{t_{1452}} = .0329$$

The mean of these three values is 0.0266, which differs from the value found by formula [2] by 0.0004. As a further check, the standard errors of the tetrad having the smallest absolute value (t_{1352}), the tetrad having the largest absolute value (t_{1245}), and the two tetrads whose absolute values lay nearest to the mean (t_{1235} and t_{2453}—two because neither lay very close to the value of the mean), in Table IIA, were also computed by formula [1].

$$\sigma_{t_{1352}} = .0440$$
$$\sigma_{t_{1245}} = .0548$$
$$\sigma_{t_{1235}} = .0469$$
$$\sigma_{t_{2453}} = .0560$$

The mean of these four values is 0.0504, differing from the value found by formula [2] by 0.0036.

The difference between the two mean tetrads is 0.0283. Assuming that the correlation between the two mean tetrads is zero, the standard error of this difference would be 0.0139. This last value is overestimated by the above assumption, as correlation is obviously present, since one group forms a part of the other. Hence, we may conclude with some confidence that homogeneity tends to decrease the relative magnitude of group factors in comparison to the general and specific factors.

It is of some interest to examine the nature of the group factors. Following Kelley,[16] we allocate each tetrad to the two pairs of variables in which it might operate as a positive bond. Tables III and IIIA show the allocation of tetrads.

The probable importance of a bond as an indicator of the existence of a group factor, may be judged roughly by the number of tetrads allocated to it in excess of the number to be expected by chance, and by the significance of the deviation of the median of these tetrads from zero. These facts have been noted in Tables III and IIIA. As a crude approximation to the standard error of a median tetrad, we may take $1.2533/\sqrt{6}$ times that of the random tetrad from the same table. For Table III, this value is 0.013, and for Table IIIA, 0.024.

In Table III, there appears to be a 1-5 bond and a 3-4 bond, with some indication of a 2-5 bond. In Table IIIA, we find evidence for a 3-4 bond again, and some indication of a 1-2 bond. Reference to the tests would indicate that the 1-5, 2-5 and 1-2 bonds could be explained by the assumption of a verbal factor. For the 3-4 bond, which appears strongly in both tables, we could

[16] *Crossroads*, 115-119.

also assume the verbal factor, operating negatively, since variables 3 and 4 are the only non-verbal ones, and every tetrad is allocated to both of the pairs of variables from which it might have arisen. It could also be explained directly by the assumption of a mathematical logic factor. A check on these alternative hypotheses can be obtained by an examination of the correlation coefficients corrected for attenuation. Aside from the r_{15} in Table I, r_{34} is the highest

TABLE III

BONDS FOR THE HETEROGENEOUS GROUPS
(N = 396)

Variables in pairs	12	13	14	15	23	24	25	34	35	45
	.040	.004	.015	.097	.015	.102	.004	.040	.034	
	.025		.035	.092	.097	.067	.035	.025	.014	
Tetrads			.034	.102	.092	.014	.059	.186		
allocated				.067			.045	.152		
				.186				.059		
				.152				.045		
Excess no. of + bonds				3			1	3		
Median tetrad if positive*				.100			.020	.052		

*$\sigma_{\text{med. } t_4}$ is of the order of .013

TABLE IIIA

BONDS FOR THE HOMOGENEOUS GROUP
(N = 87)

Variables in pairs	12	13	14	15	23	24	25	34	35	45
	.087	.001	.002	.000	.000	.001	.071	.087	.017	.010
Tetrads	.089			.002		.002	.068	.089	.017	.009
allocated	.017			.084		.003		.084	.002	
	.017			.082				.082	.003	
	.010							.071		
	.009							.068		
Excess no. of + bonds	3			1				3	1	
Median t_4 if + *	.017			.001				.083	.002	

*$\sigma_{\text{med. } t_4}$ is of the order of .024

intercorrelation in each table. Hence we may conclude that we have both a verbal factor and a mathematical logic factor in these tests. The evidence for a verbal factor is in line with that supplied by Kelley and with the more recent findings of Spearman.[17] The evidence for a mathematical logic factor, linking the geometrical analogies test with the arithmetic reasoning test, finds no support in the work of either Spearman or Kelley, and the former, in fact, specifically denies the existence of such a factor.[18]

[17] C. Spearman, Reply to T. L. Kelley, *J. Educ. Psychol.*, 20, 1929, 563.
[18] *Abilities*, 232.

The triads were computed for both groups, and are given in Tables IV and IVA.

The mean of all the triads in Table IV is 0.5731, and of those in Table IVA, 0.4175. We assume that the standard error of a mean triad is equal to $({}_{n}C_{3})^{-\frac{1}{2}}$ times the standard error of the corresponding random triad. On application of formula [10], we find that the approximate standard error of the random triad in Table IV is 0.0840, and in Table IVA, 0.2020; the standard errors of the corresponding mean triads being 0.0513 and 0.0369.

TABLE IV

TRIADS FOR THE HETEROGENEOUS Group (N = 396)

Variables a	b	c	t_{abc}	t_{bac}	t_{cab}
1	2	3	.5766	.3657	.4981
1	2	4	.6278	.3359	.5933
1	2	5	.8427	.2503	.8917
1	3	4	.5061	.5674	.7359
1	3	5	.8618	.3333	.8720
1	4	5	1.0254	.3632	.7327
2	3	4	.2948	.6178	.6759
2	3	5	.3740	.4871	.5967
2	4	5	.4088	.4875	.5459
3	4	5	.6753	.6184	.4304

TABLE IVA

TRIADS FOR THE HOMOGENEOUS Group (N = 87)

Variables a	b	c	t_{abc}	t_{bac}	t_{cab}
1	2	3	.4755	.2787	.2876
1	2	4	.4668	.2838	.3088
1	2	5	.4768	.2779	.3874
1	3	4	.2090	.6542	.6897
1	3	5	.5976	.3399	.4591
1	4	5	.4114	.3504	.4489
2	3	4	.1248	.6424	.7023
2	3	5	.2352	.3408	.4578
2	4	5	.2449	.3578	.4397
3	4	5	.6690	.6744	.2332

As a check on the accuracy of formula [10], the standard errors of the triad having the smallest value, the triad having the largest value, and the triad whose value lay nearest the mean, were computed by formula [9] for each table. From Table IV we have

$$\sigma_{t_{235}} = .0397$$
$$\sigma_{t_{145}} = .1190$$
$$\sigma_{t_{123}} = .0898$$

The mean of these values, 0.0828, differs from the value given by formula [10], 0.0840, by 0.0012. From Table IVA we have

$$\sigma_{t_{234}} = .0814$$
$$\sigma_{t_{123}} = .3886$$
$$\sigma_{t_{145}} = .2274$$

The mean of these three values, 0.2325, differs from the value given by formula [10], 0.2020, by 0.0305.

The difference between the mean triads is 0.1556. Assuming that the correlation between the two mean triads is zero, the standard error of this difference is 0.0400. This value is overestimated by the above assumption, since correlation is evidently present. Hence we may conclude with considerable confidence that homogeneity increases the relative magnitude of special factors in comparison with those of the general and group factors.

As a first approximation to the proportion of the variance of any variable to be attributed to its specific factor, we may take the value,

$$_s\sigma_x = 1 - \tfrac{1}{6}\sum_{1}^{6}(\text{all triads having variable x twice in the numerator}).$$

This formula would give an exact value if all the tetrads in the system were equal to zero within their sampling errors. Table V gives the values of these proportions.

TABLE V

RELATIVE VARIANCES OF SPECIFIC FACTORS

	$_s\sigma^2_1$	$_s\sigma^2_2$	$_s\sigma^2_3$	$_s\sigma^2_4$	$_s\sigma^2_5$	$_s\sigma^2$
N = 396	.2599	.6617	.4702	.4210	.3218	.4269
N = 87	.5605	.7591	.5110	.4861	.5956	.5825
Gain	.3006	.0974	.0408	.0651	.2738	.1556

It may be seen that variable 2, Artificial Language, has the largest specific factor in both groups. In the heterogeneous group, variables 3 and 4, the mathematical tests, are next in order, while variables 5 and 1, the distinctively verbal tests, are last. In the homogeneous group, variables 5 and 1 come second and third, with 3 and 4 last. Reference to the last column of Table V shows that in general about half the non-chance variance in the two systems is due to specific factors.

The last row of Table V shows the relative increase in the specific factor for each test, due to homogeneity. The variables in order of their relative increases are 3 (Analogies), 4 (Arithmetic), 2 (Artificial Language), 5 (Opposites), and 1 (Completion). It is apparent that this is the same as the order of increasing saturation with the verbal factor. Heterogeneity, therefore, seems to operate in this case to a great extent by increasing the verbal factor. This conclusion is supported by the very high r_{15} of Table I and the radically lower r_{15} of Table IA. It agrees also with the common observation that different racial groups in Hawaii differ noticeably in ability to handle the English language. The results of this study would seem to lend some support to Kelley's penetrating observation that the general factor is in fact itself composed principally of verbal elements and heterogeneity.[19]

SUMMARY AND CONCLUSIONS

(1) The American Council Psychological Test is an excellent instrument for the investigation of the effects of heterogeneity on the relative importance of general, group, and specific factors. Data are presented for a heterogeneous group of 396 high school seniors in Hawaii, and for a sub-group of 87 boys of Japanese ancestry.

(2) An approximation to the standard error of a random tetrad is offered, based on Kelley's formula for the standard error of a single tetrad.

[19] *Crossroads*, 18.

(3) A method for investigating the relative importance of specific factors is presented, based on a new function called the triad, whose relation to certain functions used by Spearman and Kelley is discussed. The standard error of the triad is derived, and an approximation to the standard error of a random triad is proposed.

(4) Heterogeneity increases the relative importance of group factors.

(5) Evidence is presented of the existence of a verbal factor and of a mathematical logic factor.

(6) Heterogeneity decreases the relative importance of specific factors, and does so in proportion to the saturation of the variables with the verbal factor.

The last three conclusions apply strictly only to the data of this study. General conclusions can be arrived at only through the accumulation of corroborating studies.

THE RELATIVE IMMEDIACY OF SENSORY, PERCEPTUAL, AND AFFECTIVE CHARACTERISTICS

By KERMIT W. OBERLIN, Harvard University

In confuting the so-called production theory of Benussi and other members of the Würzburg school, Koffka has argued that the whole is neither less immediate than its parts nor less original. "Die typische Form der Verbindung Reiz—Erlebnis ist nicht mehr die Empfindung (psychophysische Definition des Begriffs). So wie die Gestalten descriptiv nicht weniger unmittelbar sind als ihre Teile, so sind sie, funktionell, auch nicht weniger ursprünglich."[1]

Introspective technique is not adequate to a direct test of this proposition. The experiments of Külpe[2] and of Yokoyama[3] have shown that, as far as report is concerned, determination acts selectively upon the various attributes of sensation. The immediacy of one attribute is attained only at the cost of making the others more remote. Thus the relative immediacy of wholes and parts—of *Gestalten* and *Empfindungen*—reduces experimentally to relative immediacy of their characteristics, and this problem is the present subject of investigation. The experiment[4] was divided into two parts. The first part was designed to determine the relative adequacy of sensory, perceptual, and affective post-determination, *i.e.* of post-determination for any of the so-called attributes of sensation and for other characteristics of experience, like shape or affective value. The second part was designed to investigate the relative readiness with which reports upon these characteristics occur in the absence of any specific instructional determination.

The sensory attributes used were *hue*, *brilliance*, *saturation*, and *extension;* the others were *shape* and *affectivity*.

The materials used were pairs of bits of colored papers, approximately 1 in. square and about 1 in. apart, each pair mounted on a piece of white cardboard about 6 in. square. The *O* sat in a comfortable chair about 4 ft. in front of an exposure apparatus and peered through an aperture wide enough for full exposure of each of the two members of the pair. The exposure was made by means of an electric shutter which rested between the aperture and the colors

*Accepted for publication February 21, 1930.

[1] K. Koffka, Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse, *Zsch. f. Psychol.*, 73, 1915, 57. See also Carl Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, (no. 67).

[2] O. Külpe, Versuche über Abstraktion, *Ber. u. d. I. Kongr. für exper. Psychol.*, 1904, 56-68.

[3] For Yokoyama's experiment see E. G. Boring's note in this JOURNAL, 35, 1924, 301-304.

[4] This experiment was carried out during the year 1928-29 under the direction of Dr. J. G. Beebe-Center in the Harvard Psychological Laboratory. I also wish to thank Prof. Boring for the help he has given me in preparing his paper.

and exposed the latter to view for one second. There was an interval of 11 sec. between exposures. There were 233 pairs of colors and these were presented one after the other to each of the *O*s, with four 3-min. rest periods during every experimental hour.

In the first part of the experiment the following *O*s were used: M. E. Carver (*Ca*), A. J. Harris (*Ha*), W. D. Turner (*Tu*), and Miss D. Selling (*Se*), all of whom were research students in the Harvard Laboratory.

The *O* was given the task of comparing the brilliances of the two colors and was to report whether the left or the right color was the more brilliant of the two. At various times during this procedure he was asked, after he had given his judgment on brilliance, one of the following questions: "What were the two hues?"—"Which was the more saturated?"—"Which was the more pleasant?"—"What were the shapes?"—"Which was the larger?"

These secondary questions were chosen in haphazard order so as not to establish a definite secondary determination in *O*. On the average there were intervals of about 40 judgments of brilliance with 3 or 4 secondary questions put before a repetition of any specific question.

The *O* was also asked to tell the process and basis of his judgment in answer to the secondary questions, *i.e.* to say whether he reasoned out the answer or not, or whether his judgment was made by the mediation of an image or taken directly from the experience.

For some short trial series the data showed that the *O*s were set for the discrimination of brilliances, for the judgments were quite accurate and consistent for each *O* upon repetition of the same stimuli. This trial series was given without the secondary questions. In practically every case the introspections showed the presence of a surrogate, which resembled a memory image and lasted several seconds. Hue, as well as brilliance, was present in this surrogate. Subjective assurance of the adequacy of the surrogate was greatest with respect to hue and next greatest with respect to brilliance, a result which accords with the relative accuracies of the reports on the stimulus. The general form of the figures was present in the surrogate, but the contours of the figures were not clear. Apparently the background in the surrogate was of no quality. The figure was very distinct, but the background was insignificant.

In the formal experimental series where the secondary questions were asked, the introspective reports showed the presence of surrogates in almost all cases. It is safe to say that these surrogates were the basis for every kind of secondary judgment. Furthermore, the reports indicated a strong tendency toward a cumulative predisposition, for, upon repetition of the secondary questions the *O* came finally to be predisposed not only toward brilliance but also to many of the other attributes that were required. Yokoyama has told us nothing about this in his experiment. It would be interesting to know whether his *O*s were more easily able to give a report on his secondary questions at the end of his experiment than at the beginning.

In this case where the *O* became predetermined in the direction of the secondary questions as well as the primary, the surrogates sometimes did not appear. When some judgment was unusually difficult and took longer than

usual, however, then the surrogate always appeared and enabled *O* to make any other judgments that were required.

The judgments of affection came by means of surrogates. In one case they were reported as "slow." In two other cases they were definitely associative judgments. Apparently affective judgments are more mediate than the others.

In the case of *Ca* the judgments came so quickly and readily that it was impossible for him to tell anything about the process of making the judgment. The judgments 'just seemed to come' and were without conscious antecedents. Toward the end of the experiment, however, he became aware of taking 'the stimulus as a *Gestalt*' and of reacting to the whole configuration with the two colors as figures upon a ground. No surrogates were involved in making the judgments upon the brilliance. The judgments upon saturation involved surrogates about fifty percent of the time and the other judgments were made practically always from a surrogate.

Ha was able to tell more about the process of making the judgments. Some of his judgments seemed to come by way of a process of reasoning. For example, here is part of one of his introspective reports: "The judgment process came like this: Right was rich but a tint. Left was rather poorly saturated but on a median level. Then made up my mind that the tint was more saturated than the other, *i.e.* taking into consideration the brilliance component in judging saturation." *Ha*'s affective judgments were made associatively and always from a surrogate. After one affective judgment he remarked: "Not a real affection but a sort of cognitive thing, *i.e.* a recognition of colors of things I usually don't like." Another time he said, "The affection is a very mild sort of thing, not real. *Unpleasant* seems to be merely that I prefer other colors to it." Surrogates seemed to be present in all of *Ha*'s judgments with the exception of the judgment of brilliance. He also had many doubtful judgments and judgments of *equal*, where he could not make a positive decision toward either of the two members of the pair, in respect to brilliance, the primary question. He was, however, very certain of his accuracy in the judgments that he made.

Tu reported that surrogates were present in every case and all the secondary judgments were made from them. He stated that "affection does not come unless asked for" and that then "it builds up very slowly;" in some cases it was "just a slight preference." He, as well as *Ca*, could tell nothing about the process of making the judgment.

Se could not give any judgments on *size* or *shape* because of her habit of squinting her eyes in order to eliminate the contours and to enable herself more easily to make a judgment on brilliance and saturation. Her method of making the judgments was quite different from the methods of the other *O*s. For example, she said that, when the two colors were seen, she projected two imagined parallel scales, one for brilliance and one for saturation, that she then localized the two colors on these two scales as to brilliance and saturation. She used a fairly detailed associative method of judging in the case of affection, a method which involved a judgment of hue and other judgments of associative intermediaries before the judgment of affection. Where there was a marked difference her judgments of affection came quickly and the consciousness of the

hues was present apparently as soon as the exposure was made. Where the difference between hues was small, however, the affective judgment was made from a surrogate. She said: "I am always looking for brilliance and not conscious of looking for anything else until asked for it. After being asked, sometimes I can answer right away but am not conscious of knowing that before. If the answer does not come at once, then I try to look back or recall a surrogate."

There was a predominance of images or some other kind of surrogate for the judgment in nearly every case. In respect to the resemblance of the surrogate to the stimulus, the surrogate was usually described as a memory image with a great variability in vividness. In the few cases where the judgment came immediately, there were, of course, no surrogates to describe.

The first part of Table II summarizes the occurrence of surrogates in this experiment.

In the second part of the experiment the procedure was somewhat changed. Only about one third of the total number of stimuli were used. Here, there were four new *Os*: D. W. Chapman (*Ch*), P. E. Huston (*Hu*), Hyung Kim (*Ki*) and Johann Twersky (*Tw*). The instructions to the *Os* in this part of the experiment were as follows: "You will be presented with two stimuli simultaneously, for one second. You are to tell me the difference between the two—*any* difference. Try to be passive in this experiment and do not look for any particular difference. Report as quickly as possible. In the introspection, report on the basis of the judgment and the process of the judgment."

As soon as possible, after each judgment of a difference was given, the *Os* were asked to give their introspections as instructed. They were not allowed to ask any questions but were expected to report with no further instructions.

The introspective reports of *Ki* and *Tw*, foreigners, were written out by *E*, but the reports of *Ch* and *Hu* were dictated into a dictaphone.

The problem here was to find what differences would be experienced when the *Os* were not predetermined toward any particular difference. Would perceptual and affective characteristics ever be reported when not asked for? Would all of the sensory attributes be noticed?

Under these instructions *E* expected to find that those properties of the stimuli in which they differed the most would be reported most often, and that the largest difference would be noticed most quickly and most easily judged. Because the instructions did not limit the *Os* much, large individual differences in the reports were expected, for the *Os* would bring to this experiment their own peculiar predeterminations toward colors.

The data from *Ch* indicated that in order to experience affection this *O* required a special affective determination. He gave no judgments of differences in affection until the first third of the material had been presented. Then in his introspective report we find: "It occurred to me at this point for the first time that perhaps I could find differences in P and U between the stimuli—as far as I can recall none of them has appealed to me as P or as U—perhaps that is determined by the set." For the remaining stimuli, however, he gave judgments of difference in affection in more than forty percent of the cases. *Ch*

seemed to be the keenest observer and found more differences than any of the other *Os*. Further on in his introspections he said: "I find that in general the significance of the difference is immediate, but it is impossible in many cases to state immediately what the difference is." For him, *size* and *position* were immediate and easily statable, but for differences in hue, saturation or brilliance, he had recourse to the color pyramid in imaginal terms. He also said: "I think that after a number of these observations, a very few of them in fact, I acquire, without actually willing to do so, a determining tendency to report differences in hue, and it probably influences any judgment and certainly influences me to look for hue differences wherever I expect tham to exist." These reports also revealed a searching, on the part of *Ch*, of the surrogates for any differences that he could think of and that did not come immediately. On the whole, we can say that, even with such a large exposure-time, *Ch* based a very high percentage of his total judgments on the surrogates.

The introspections of *Hu* were not so complete as those of *Ch*. *Hu* noticed, however, the differences in affection from the beginning. In general, his affective judgments seemed to be more difficult for him to make, and he usually reported them as coming last in a temporal series. His reports indicated that he always judged the affectivity of the colors associatively. He even reported some physiological accompaniments of the affection, and they may possibly be considered as some part of the surrogate for this experience. For example, here is a report: "Affection came a little later and was accompanied by a feeling of irritation and strain in the abdominal region because of the slight difficulty in making the judgments." Then again: "Both were extremely P and delicate and aroused a warm agreeable feeling." He resorted to the use of the color pyramid in imagery a great part of the time and explained it thus: "When the differences are large, the judgments are made easily from the stimulus itself; but, when the judgments are difficult to make, then reference must be had to the color figure to get an accurate discrimination."

Ki reported differences in affectivity for almost all of the stimuli. He experienced very few differences for each stimulus as compared to the two preceding *Os*. Here was a case in which there was no accumulation of various determinations. *Hue*, *size*, and *affection* were the attributes which were by far most often noticed. In many of the cases the affective judgments were made associatively. For *Ki* surrogates were present throughout the experiment.

For *Tw* the protocols showed one judgment each of brilliance and vividness throughout the experiment; all other judgments were for size and affection only. *Tw* said that his judgments were intuitive and subconscious and that he could not, therefore, give any information about the process of the judgment. The judgments came easily. Nearly always imagery was present and the judgment on one member of the pair of colors was taken from the surrogate, while the judgment on the other member was taken directly from the stimulus. He said that the judgments based directly on the stimulus came more easily than those derived from the surrogate. He also said that if he was more attentive he could judge on the stimulus, and that otherwise it was necessary for him to reconstruct a surrogate for a basis of judgment.

Table I shows which *Os* reported spontaneously various ones of fourteen sensory, perceptual or affective characteristics.

When descriptions of the surrogate, following the report on the secondary questions, were required, the *Os* gave, essentially, the following report: The thing that persists is a hue with a certain brilliance; other things may persist but only if they are striking. The surrogate as a whole begins, immediately after the presentation, to narrow down to those things that are striking, and,

TABLE I

THE DIFFERENCES REPORTED BY THE DIFFERENT *Os*

O	Hue	Brilliance	Saturation	Affection	Size	Shape	Texture	Position	Spottedness	Glare	Delicateness	Height	Width	Wrinkledness
Ch	X	X	X	X	X			X				X	X	X
Hu	X	X	X	X	X		X			X	X			X
Ki	X	X	X	X	X	X		X	X					
Tw				X	X									

when the secondary question corresponds to one of these characteristics, subjective assurance is high; but when the secondary question does not correspond to a striking characteristic, report becomes impossible.

The second half of Table II summarizes the surrogative mechanism in these spontaneous characterizations.

CONCLUSIONS

There are great individual differences in the *Os* as to what they can experience in color stimuli without predetermination in the form of instructions. The differences in the attributes of hue, saturation, and brilliance, or at least the finding of these differences, depends upon the familiarity of the *Os* with these attributes before coming to the experiment.

On the basis of the data gathered from both parts of this experiment, we can conclude that there is no non-temporal difference between sensation and perception so far as immediacy is concerned. What the data show is that there is no difference in the immediacy of sensory characteristics as against perceptual characteristics. Because affection has proved, however, to come associatively in so many cases, and so many times with an elaborate train of surrogates preceding the judgment, it must be concluded that the affective judgment is more mediate than the other judgments.

TABLE II

ANALYSIS OF SURROGATION FOR ALL *Os* IN BOTH PARTS OF THE EXPERIMENTS AND FOR THE SEVEN MOST IMPORTANT CHARACTERISTICS

	O	Brilliance	Hue	Saturation	Size	Shape	Affection	Position
Part I	*Ca*	No surrogate	Surrogate in all cases	Surrogate in half the cases	Surrogate in all cases	Surrogate in all cases	Surrogate in all cases	
	Ha	No surrogate	← Surrogates →				Associative judgment	
	Tu	No surrogate	← Surrogates →				slow	
	Se	No surrogate	Often immediate; surrogate when delayed		No data	No data	Surrogate; associative judgment	
Part II	*Ch*	Mediate via color-pyramid			Immediate and easy		After self-determination in 40% of cases	Immediate and easy
		← Further search of surrogates for differences →						
	Hu	Mediate via color-pyramid for difficult judgments			Size and texture frequent. Often immediate	Shape not reported	Delayed via surrogates	
	Ki	Brilliance not reported		Saturation not reported			Associative judgment	
		← Always a surrogate with few properties at a time →						
	Tw	← No judgments →			Surrogate; easy	No judgment	Surrogate; easy	

THE EFFECTS OF PRACTICE ON FATIGUE

By J. A. Glaze, Texas Christian University

Several months after my first experiment on fasting,[1] it occurred to me that the long period of practice preceding the fasts might yield interesting results independently of the fast itself. One of the tests consisted of writing the letters *ab* as fast as possible for twenty minutes. Accordingly I re-graphed the practice-periods, comparing the first five-days' work-curve with the curve for the second five days, and both with the remainder of the practice-period. I also

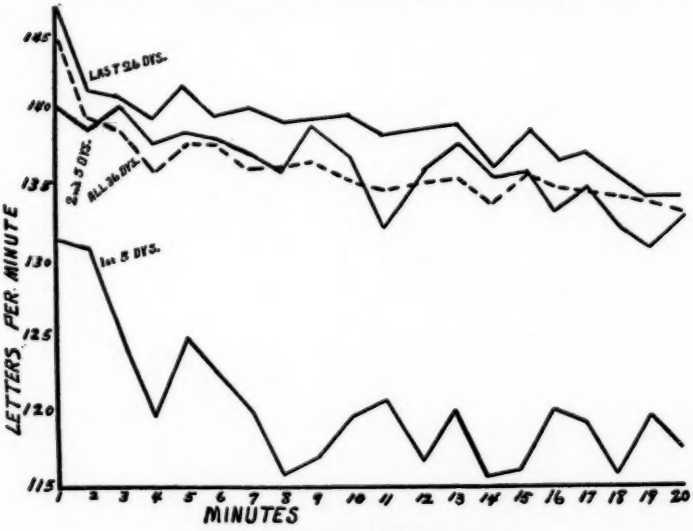

Fig. 1. Curves of Work for A
Based on writing *ab* 20 minutes daily for 36 continuous days

compared the first seven and the first ten days of work with the second seven and the second ten days, and both of these with the remainder of the practice-period. I wished to see whether appreciable differences occurred. The first comparison (the two five-day periods of practice) yielded the most conspicuous results. These I present here. Only that part of the practice-period up to the time of the fasts is considered. The longest of these practice-periods was thirty-six days and the shortest twenty-three. These periods represented a

* Accepted for publication June 17, 1930.

[1] J. A. Glaze, Psychological effects of fasting, this Journal, 40, 1928, 236-253.

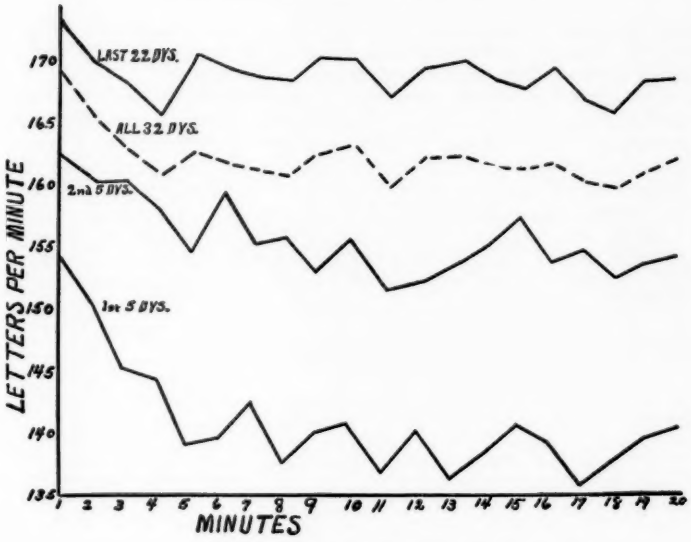

FIG. 2. CURVES OF WORK FOR B
Based on writing *ab* 20 minutes daily for 32 continuous days

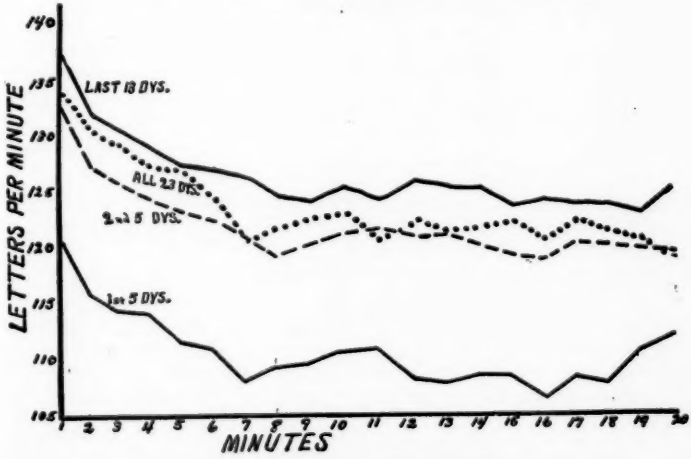

FIG. 3. CURVES OF WORK FOR C
Based on writing *ab* 20 minutes daily for 23 alternate days

fairly long and consistent task, averaging a month of actual work for each subject. A and B practiced every day for 36 and 32 days, respectively, while C practiced about every other day, his total being 23 days.

The results appear in the three accompanying graphs. In each instance the curve of work for the first five days drops rapidly during the first few minutes of work. During the second five days of practice the curve continues to drop in the early minutes of practice, but the drop is not so pronounced. The most conspicuous difference is the work-curve of A. The second five days of work tell a different story from that of the earlier period.

Dodge has contended[2] that relative fatigue is not a limitation of human efficiency, not exhaustion of the individual, but rather a preventive of exhaustion. We might add that the effects of repeated performance of such a task as ours tend to mask the manifestations of fatigue. Adaptation and the attainment of a fairly high degree of efficiency proceed regularly, eliminating the result that is manifest in the early practice-period.

Thus it appears that work-curves for the entire practice-period fail to tell anything like the whole story. Other writers have incidentally mentioned this fact; but no one, to my knowledge, has adequately illustrated it.

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[2] R. Dodge, The laws of relative fatigue, *Psychol. Rev.*, 24, 1917, 89-113.

APPARATUS

A RELAY SYSTEM FOR TIMING OR COUNTING[1]

By JOHN O. KRAEHENBUEHL and MAX A. FAUCETT, University of Illinois

It is often necessary to obtain the accurate count or timing of an operation when the source of energy for operating the recording device is very small. The apparatus herein described was used in a study of the influence of personal habits and the changing of test-switches on the accuracy of the meter-tester, where it was necessary to time a definite number of revolutions made by the disk of a watt-hour meter. In this case a recording device attached to the meter would introduce a degree of error of such a magnitude as to make the results worthless; and only by using a beam of light was it possible to record the time without disturbing the energy-balance of the system measured. A beam of light was permitted to pass through a hole in the meter onto the sensitive photoelectric cell, and the electric impulses from the cell, properly amplified and relayed, were used to start and stop a cycle-counter.

This assembled amplifier and relay circuit may be used in a variety of problems, such as studies of reaction-time or of other performances where the interval of time to be measured may vary from a few sigma to any number of minutes. As a matter of fact, there is no kind of interval or series of impulses that cannot be so recorded. It thus recommends itself for use in all types of tapping tests and in every form of pursuit-meter where account has to be taken of both the total period and the number of breaks in the contact.

The complete system consists of the following units arranged in order; the exciting unit, the amplifier, the master relay, the holding circuit, and the isolating relay. Each unit may be used separately or as a part of the whole device. This flexibility of arrangement does away with numerous pieces of idle equipment.

A. The Photoelectric Cell. The beam of light may be projected on the cell-surface either by means of reflected rays from a small mirror in the path of an arc-lamp or by direct rays. In either case, the surface of the cell should be illuminated to twenty-five foot-candles when using a single-stage amplifier.

B. Amplifier. The amplifier may consist of one or more stages of resistance coupling, using a bias-battery instead of a condenser between stages. When the impulse is from direct current, the ordinary amplifier will not function satisfactorily. The single stage will prove satisfactory in most cases and requires few batteries. It is well to keep in mind the fact that any source of current in the order of five or ten micro-amperes may be used as the exciting source in place of the photoelectric cell.

[1]The authors wish to acknowledge suggestions regarding applications to psychology from Dr. C. R. Griffith, Director of Research in Athletics and Associate Professor of Educational Psychology.

Quite frequently measurements are rendered impossible by the fact that the current is below the magnitude necessary to operate very sensitive instruments. By disconnecting the amplifier-links it is possible to measure the output in the plate circuit with a milli-ammeter. The amplifier so isolated may be used as a separate piece of apparatus and at the same time by a simple connection be made available for the relay-system.

C. Master Relay. This relay may be of the sensitive polarized type or merely a simple sensitive relay. Either of these will complete a local circuit which will operate any auxiliary apparatus desired. By means of this relay the impulses may be counted on an electrical counter or, if a timing device is placed in the relay output, it is possible to determine the period of time elapsing between single impulses. The modern electric clock and the normal lighting system prove very satisfactory for timing if the interval of time is several minutes; but when a high degree of accuracy is required, a synchronous timer recording ½-cycles may be used. It may be found desirable to isolate the master relay from the alternating current, in which case the output of the master relay may be passed directly to the isolating relay.

D. Holding Relay. While the master relay was satisfactory for timing between impulses, it was found necessary to introduce another device when the

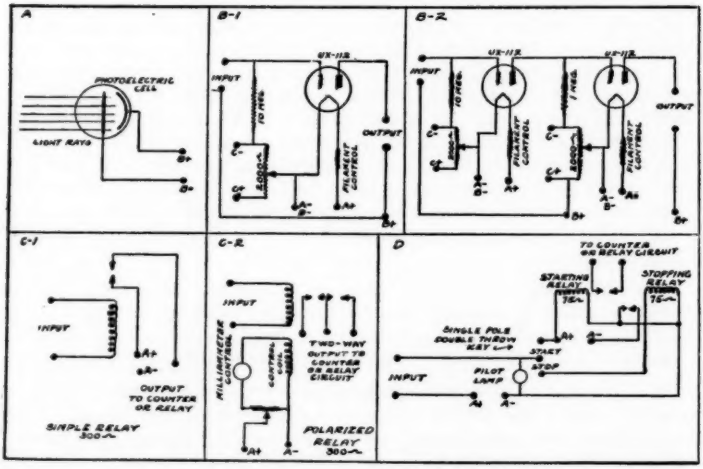

FIG. 1. ESSENTIAL UNITS FOR RELAY AND AMPLIFIER SYSTEM

(A) Photoelectric Cell, using the B-battery specified by the manufacturer. (B) Resistance Amplifiers, using A- and B-batteries to obtain the proper voltages specified for the tube and in each case using 22½-volt C-batteries. (C) Master Relays should be limited to one ampere on the contacts; in the polarized relay use a 1½-volt adjusting-battery. (D) Holding Relay. There should be a difference of 2 volts between the two A-batteries.

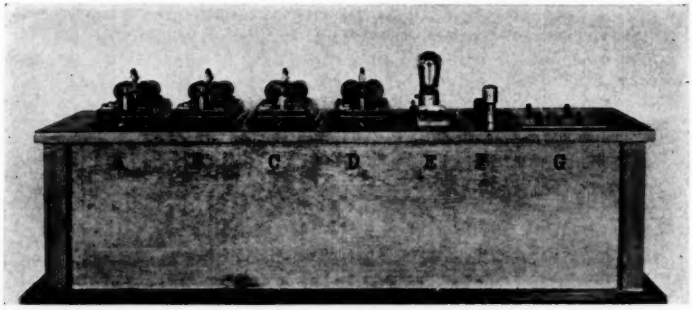

FIG. 3. ASSEMBLED FOUR-UNIT RELAY
(A) Isolating Relay, (B) and (C) Holding Relay, (D) Master Relay, (E) UX-112 Tube and Grid Resistance, (F) Front—Pilot-Light, Back—Bias-Battery Control Knob, (G) Switches for Battery Circuits.

timing period was to include several of the impulses. The device consists of two 75-ohm relays so connected that one relay is depolarized by means of a reversed battery. To start the operation, the switch is closed after a flash of the pilot-light and then released, and the flashes are counted which represent the impulses being transmitted until the desired number have appeared. The switch is then reversed and the next flash will open the circuit disconnecting the timing device. This presupposes that the interval of time between individual impulses is long enough for the operator to throw the switch. It must be borne in mind that the operator does not start and stop the cycle-counter or timing device. He merely sets the switch at the proper time and the impulse starts and stops the timing.

E. Isolating Relay. This is a 75-ohm relay of a rather insensitive type as it always functions on a local circuit of batteries.

Fig. 1 is a set of diagrams showing the various units described and their connections as a unit. Fig. 2 shows the connected relay-system in its simplest

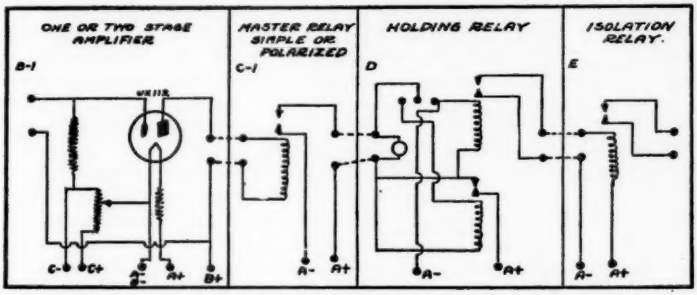

FIG. 2. FOUR-UNIT RELAY
Schematic diagram and the connecting links between the various units. Use A-, B- and C-batteries as specified in Fig. 1.

possible arrangement. Fig. 3 shows the assembled four-unit relay as laid out in Fig. 2. The compartment upon which the units are mounted contains the batteries.

The mechanical lag of the apparatus is constant. When investigated on an oscillograph with a 1000-cycle timing wave, it was found that the difference of time between the impressed impulse on the amplifier and the delivered impulse of the isolating relay was .002 sec. This is a constant error.

The flexibility of the device with its sturdiness and reliability of operation commends it for all investigations that require counting or timing. When used with the photoelectric cell, measurements may be made which in no way disturb the energy-balance of the device under observation. The cell will operate on white or colored light.

APPARATUS FOR STUDYING EYELID RESPONSES

By L. W. Allison, Peabody College[1]

The photographic technique of studying eye-movements, while offering certain advantages of refinement, has also practical disadvantages. The initial cost of the equipment is fairly high, as is also the operating cost when the nature of the work requires a large number of records. It seemed worth while, therefore, to devise a piece of apparatus less expensive but satisfactory for most purposes. The apparatus described grew out of a series of researches on various phases of eyelid-responses which are serially stimulated at intervals ranging from ½ to 4 sec.

A. Stimulus Unit. Fig. 4 shows the simple mechanical device for giving and recording the onset of the stimulus. The upright pieces A and A′ are 6 in. high and serve as supports for the two wooden rollers B and B′, around which belt C passes. This belt is 1¾ in. wide and 30 in. long and is made of oilcloth (smooth side up), used because the holes cut in it do not wear larger or ravel out at the edges. Small openings 1/16 by 3/4 in. are cut through the belt, allowing a metal finger D to come into contact with a brass plate E beneath the belt and on top of bracket F, thus closing the circuit. D is made of a piece of spring steel 5 in. long by 1/8 in. wide attached to bracket G. It has sufficient elasticity to allow the belt to move freely and enough rigidity to insure contact with plate E through the openings in the belt. H is the positive connection and I the negative. By means of an electric marker in the circuit, the onset of the stimulus is recorded on a kymographic record.

The power for driving this unit is furnished by the constant-speed motor of the Renshaw polygraph. Any good constant-speed motor would serve. The motor is adjusted to move the belt one inch per second. The time between stimuli is thus determined by the distance on the belt between the openings. But time-scales were recorded on all records with a 50 d.v. electrically-driven fork. A ½-sec. interval is thus obtained by allowing ½ in. between the openings in the belt; a 1-sec. interval by 1 in., etc. In order to determine the constancy of the speed of the motor (or the general reliability of the time-record), fifty counts of the ½-sec. interval against the time-scale were made at random from ten records. The mean count was found to be 25 (500σ). On no count was the deviation found to be more than 1/50 sec.

B. Responding Unit. Fig. 5 shows the mechanical device for recording the eyelid-response. It consists of a pair of spectacle-frames made of heavy wire ⅛-in. in diameter, which could be bent to conform to the shape and size of the head. To the left frame was attached a small cuff (A in the figure) which held in place a piece of rubber (B) .4 x .4 x .6 in. Through the center of this rubber a piece of german-silver wire (C), No. 16, runs parallel to the left frame of the spectacle. D is an adjustable fulcrum on which a lever (E) is perfectly balanced, this lever being also made of the same german-silver wire. Since this lever is balanced when at rest on fulcrum D, the slightest movement at F will bring the distant end of the lever into contact with C and thus close

[1] The writer is indebted to Dr. Joseph Peterson for valuable assistance in the preparation of this manuscript.

the circuit. By means of an electric marker in the circuit, the instant of the response is recorded on the same kymograph-record as that on which the onset of the stimulus is recorded. The fulcrum D can be slipped forward or backward on the base G. Base G is soldered to the frame at N and is loose at the other end (R), except for a small ring which fits around the frame and the base which holds fulcrum D in position after it has been properly adjusted. This adjustment is essential, due to the wide variation in size and shape of the heads of the various subjects. E is a small piece of adhesive plaster 1/12 x 1/3 in., by means of which the lever is attached to the subject's left eyelid just above the free margin. This lever is so perfectly balanced that, after a few minutes of adaptation, the subject is unaware of its movement with the eyelid.

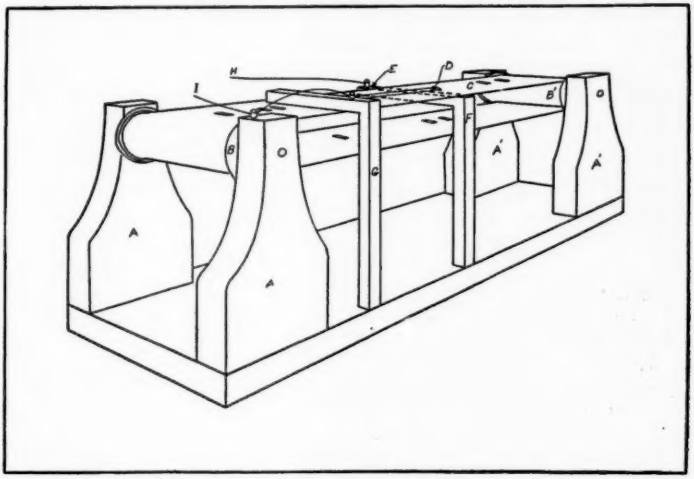

FIG. 4 STIMULUS UNIT

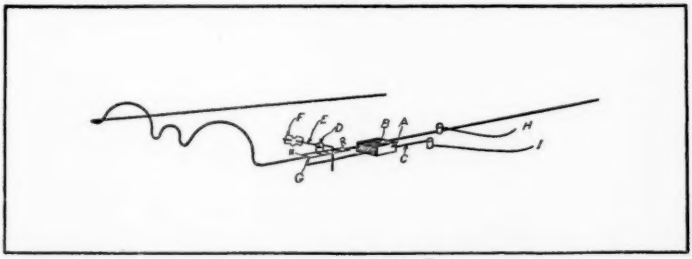

FIG. 5 RESPONDING UNIT

A DEVICE FOR READING CONTINUOUS GRAPHIC RECORDS

By G. L. Freeman, Yale University

The reading of continuous graphic records is usually a slow, awkward and unsatisfactory process. Below is shown a device which greatly relieves and improves the customary procedure.

Two rollers (a, a′) move the record over a polished wood surface (b) in a manner similar to the action of a kodak film. Counter-clockwise turns of the crank (c) draw the paper under the celluloid reference-guides (d, d′); clockwise motion allows the reader to return to points earlier in the record. The reference-

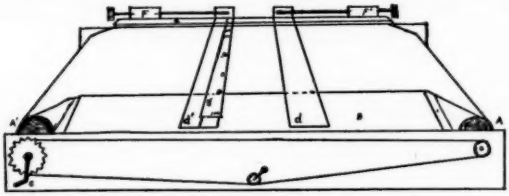

Fig. 6. Device for Reading Graphic Records

guides are placed perpendicularly to the metal bar (e) along which the record moves. The distance between the guides can be altered by means of micrometer screws (f, f′). One reference-guide carries a celluloid reading scale (g) graduated for 100 mm. above and below a zero point. Brass grooves make it possible to adjust this zero position to any record.

The device is equally suited to polygraphic, photographic and kymographic records and it will accommodate widths of paper up to eight inches. In practice one inserts a record roll over roller (a), threads the paper under the reference-guides and fastens the end to roller (a′). The use of the reference-guides is determined by the character of the material to be analyzed. If, *e.g.*, it is the number of reactions per minute, the user simply has to bring the first small mark in line with the first reference-guide and adjust the second guide so that it is over the next time-mark; then he reads the number of oscillations in the performance-curve which are included between the two guides. If the record originally moved at a constant rate, it will not be necessary to change the position of the guides for subsequent periods.

Equivalent readings may be obtained from a number of simultaneous curves by setting the guides to conform to the time-unit. If the height of any behavior curve is to be read at stated intervals, the zero position of the scale is set at the beginning of the record and the position of the curve is noted at every passage of the time-mark under the guide.

Since the device is operated by hand, the rate of reading can be suited to the type of record and to the individual performer. Turning the crank with one hand and writing with the other, the operator is able to achieve a fairly rapid pace with most continuous records. To insure accuracy, the paper must run evenly along the bar (e). At the conclusion of the reading, the record may be slipped off the roller, or it may be quickly wound back on the opposite roller.

The chief advantages are (1) greater compactness (the dimensions of the apparatus are 10 in. by 20 in.), (2) greater accessibility (discontinued reading may be resumed without difficulty in finding the place), (3) greater accuracy (the measuring instruments are applied at the same angle and height), and (4) greater speed (the rate of reading may be doubled).

AN INEXPENSIVE ROTATION TABLE

By KARL M. DALLENBACH, Cornell University

We have constructed in the Cornell laboratory an inexpensive rotation table from parts of scrapped automobiles. As shown in outline in Fig. 6, half of a split differential housing forms the base of the apparatus, and a rear wheel, to which a three-ply circular wooden top (36 in. in diam.) is fastened by lag screws, serves as the rotating unit. We used a wheel with a floating axle, *i.e.* one that revolves upon the axle housing, so that the downward thrust of the weight upon the table is carried by the conically shaped lower bearings. After removing the floating axle and the internal brake band, we welded the two housings together. We loosened the external brake band so that the wheel turned freely, but we retained it so that it might be used in retarding or suddenly stopping the rotation. The far end of the brake rod connects to the brake lever of an automobile, which is securely fastened to the wall of the experimental room.

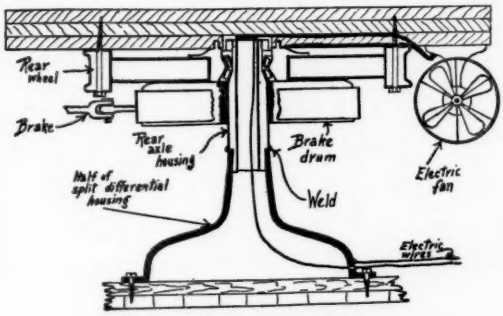

FIG. 6. A ROTATION TABLE

The table may be rotated satisfactorily by hand, but a more convenient and exact method is offered by means of an electric fan which is fastened, as shown in the outline drawing, to the under side of the table-top. The wiring of the fan is simple. The 'hot' wire is connected by brush contact to an insulated circular copper disk, which is in turn connected to the fan; the 'cold' wire is attached to the steel base of the apparatus, and the circuit is completed by a wire leading to the fan from the metal hub of the wheel. The speed of rotation may be varied by changing the resistance in the fan circuit.

This device may be put to various uses. It may be used in rotation experiments by fastening a chair (in which the observer may be seated) or a board (upon which he may lie—and to which he should be strapped) to the top of the

table by thumb screws; it may be used, since it rotates freely and noiselessly, to present the stimuli in psychophysical experiments with lifted-weights; and it may be used, since it is strongly and solidly constructed, in experiments with animals, in particular anthropoids.

APPARATUS NOTES

A New Technique for Recording Sound Localization

I have already called attention to the fact that we have obtained many results in the field of sound localization without carefully scrutinizing the possibility of improvement in the technique of recording.[1] In comparing results obtained by the method of *visualization* with those obtained by the *pointing* method, while in a sense this work was done with an insufficient number of subjects, there was a definite indication that the method of visualization was inferior to the pointing method both in the average total displacement and in the percentage of correct judgments. The subjects believed, however, that the visual method was the more accurate of the two.

The method of visualization has been such a great favorite, among experimenters, moreover, that every effort should be made to improve this technique. In a recent thesis developed under my direction I suggested the use of luminous paint.[2] The difficulty with ordinary luminous paint is that it has to be exposed periodically to light in order to remain luminous. We therefore tried radium paint which proved entirely satisfactory. The numbers indicating the main positions in degrees were painted about 2 cm. high with marks at intervals of five degrees on black cardboard which was bent around in the form of a semi-circumference with a radius of about one meter. Another sector in the sagittal plane could also be used in the same way. Instead of guessing as to the direction of the sources of the sound or at best recalling it from a visualized scheme the numbers could thus be called off from the luminiferous series in front of the observer in the totally dark sound-proof room. Although our conclusions so far are entirely empirical we feel confident that there is a distinct improvement in the warranted assurance of the reports. We are now undertaking further investigations which will be distinctly comparative in nature attempting to establish the possibilities of this technique.

University of Iowa — Christian A. Ruckmick

Use of the Neon Lamp in Phonophotography

The short duration of the single flashes of a neon lamp, as well as the extremely high frequencies which such a lamp will follow, led us to seek a means whereby the flashes could be photographed. It was found that if a hole is melted in an ordinary neon lamp,[3] a small quantity of mercury injected and the lamp refilled to 10 mm. pressure with a commercial gas known as B-10 (a mixture of neon, helium and argon and available at all neon sign companies), the lamp will then glow with a faint bluish light to which photographic paper and film

[1] C. A. Ruckmick, Sound localization: a comparison of methods, *Proc. Iowa Acad. Sci.*, 32, 1925, 399-400.

[2] Anna Mathieson, Apparent movement in auditory perception. (Soon to be published.)

[3] Suitable lamps can be secured at approximately fifty cents each from the General Electric Vapor Lamp Co., 410 Eighth St., Hoboken, N. J.

are exceedingly sensitive. The lamp thus prepared can be made to flash in synchronism with a sound wave by connecting it to the output of an amplifier utilizing a plate voltage of the order of 350 volts on the last tube. The lamp used in this manner has been found very effective when used in conjunction with Metfessel's strobophotograph, a device which directly graphs frequency with respect to time.[1]

The advantages of photographing the flashes from a neon lamp in preference to techniques using optical levers and tambours are that the former (a) easily follows all frequencies within the range of the human voice, (b) has practically a linear characteristic over the frequency range of speech and song (if a good amplifier is used), and (c) permits the use of amplification for the recording of very weak sounds. These features make it possible to take consecutive records of all frequency changes throughout an entire vocal rendition.

We are also using the lamp very effectively in other pieces of apparatus as a time-line by omitting the amplifier and connecting the lamp in series with the secondary of a small induction coil, the primary of which is in series with a dry cell and an electrically driven tuning fork of known frequency. The neon lamp flashes only on the break of the current, hence giving as many flashes per second as the frequency of the fork used. By placing a narrow slit, extending across the width of the film, between the moving film and the lamp, parallel time-lines are secured.

JOSEPH TIFFIN
University of Iowa MILTON METFESSEL

A MANOPTOMETER

The writer has used Parson's[2] manoptoscope in an attempt to secure data which indicate native handedness. It consists of a conical-shaped sighting tube and an exposing device which is placed "about 2 feet in front of" the subject, who indicates the letter (R-on-the-right or L-on-the-left) that he sees. Sources of error have been found and an attempt has been made to construct an instrument which will be relatively reliable and valid.

The manoptometer has a small wire attached to the top of the easel and to the sighting tube which makes it easy to keep a distance of two feet between them. The sighting tube tapers to a circular aperture of 15/16-in. Pictures of a cat, bird and boy are used instead of the letters L and R and the fixation disk, since a test of handedness is frequently given to pre-school children and to early elementary grades. The pictures of the cat and bird have been placed on separate slides so that they can be moved independently nearer to, or farther from, the center. This completely eliminates the necessity of having S move nearer or farther than two feet. The chance order followed in moving the slides toward the center of the easel and the substitution of the pictures for the letters R and L have eliminated some of the errors of Parson's apparatus. The back of the easel has been divided into cm., making possible quantitative results. The changes seem to justify the statement that we have designed a new measuring device which we have called a *manoptometer* because it can be used to secure quantitative results in studying the facts of eyedness and handedness.

Eastern Kentucky State Teachers College NOEL B. CUFF.

[1]Milton Metfessel, The strobophotograph: A device for measuring pitch, *J. Gen. Psychol.*, 2, 1929, 135-139.
[2]B. S. Parson, *Lefthandedness*, 1924.

NOTES AND DISCUSSIONS

Some Uses and Misuses of the Term 'Aesthetic'

Scepticism as to the relevance of experimental methods to the peculiar problems of aesthetics is widespread and, in spite of valiant but sporadic efforts to dispel it, shows no evidence of abating. Pessimism as to the possibility of an experimental groundwork is, however, in all likelihood without factual warrant. The present backwardness is, in my opinion, the outcome solely of an arbitrary definition of the term 'aesthetic,' and an artificial delimitation of the field (plus the inevitable sequel, misguided methods of attack). Illustrative examples of such misleading and distorted usage may be drawn from a recent issue of this Journal, *e.g.* in an exploratory study of the effect on function of formal *versus* self instruction, Anderson[1] identifies "the impersonal valuation of objects as conforming or failing to conform to some conventional standard" or "approval and disapproval" as the typical aesthetic attitude (an obvious confusion of the aesthetic with the critical). A second instance of arbitrary narrowing of the field occurs in a suggestive study of the least perceptible difference between two silhouetted profiles, by White and Landis.[2] Duped by the chance discovery of a correlation between deviation and dislike,[3] the latter authors exuberantly forecast the discovery of an 'optimal' human profile, to serve as an artistic standard of comparison; heralding further the wide application of their method, to landscape and design as well as to faces,[4] and the establishment at last of a genuine quantitative method of 'aesthetics' (p. 435)!

The origins of these two correlative misconceptions are readily decipherable. The first, the identifying of aesthetics with the science of criticism and of norms, is obviously a hangover from the age when pedagogues and philosophers busied themselves in parcelling out the provinces of knowledge. A companion-piece was needed for Ethics, and therefore, with supreme *a priori* confidence, Aesthetics was pigeon-holed alongside it, as a normative or evaluative science. The second fallacy, the confusion of aesthetic appreciation with mere pleasurable feeling, harks back to nineteenth century hedonism, and the rigid affective dichotomy that limited feeling to the agreeable-disagreeable antithesis. A legacy of the Victorian era, it forms a fitting pendant to the 'mauve decade,' with its flair for mere prettiness and tepid sentimentality. The healthy reviving zest for the grotesque, the tragic, the ugly, the uncanny, cuts clean across it.

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[1] O. D. Anderson, An experimental study of observational attitudes, this Journal, 42, 1930, 345 ff., esp. 355-359.

[2] R. K. White and C. Landis, Perception of silhouettes, this Journal, 42, 1930, 431-435.

[3] Every user of the Binet test is familiar with the analogous pseudo-aesthetic choice by the 5-year-old child of the 'prettier' of a pair of heads. The deviation from the placid, conventional and commonplace standard is rejected with illuminating comment, as unnatural, strange, 'cross,' terrifying.

[4] A somewhat similar principle is utilized in the Meier-Seashore Art Judgment Test (developed by N. C. Meier with the coöperation of C. E. Seashore, Bureau of Educational Research and Service, University of Iowa, 1929), in its 125 paired reproductions, consisting each of a standard and a variant.

Now actually, psychologist and layman will agree, 'conformity with a standard,' (Anderson's thesis) instead of furnishing the basis of aesthetic feeling, or furthering aesthetic appreciation, too often acts as an effective and all but insuperable obstacle, which must be somehow hurdled or evaded before genuine aesthetic (as distinct from practical or critical) perception can get under way. Up to a certain point, it is true, familiarity with form or structure (with the tricks and turns, key-changes and bandying of motifs, of the classical 'sonata-form,' or with the painter's legerdemain of light and line and balance) may augment one's rapport with the content of an art-work, and enhance one's affective apprehension of the whole. Yet human frailty—the ineradicable law of use and tuning of the nervous system—all too speedily converts this advantage into a handicap. Repetition (or familiarity) not merely dulls the splendors of the known, but throws one out of gear when the work of a new genius—a Franck, a Van Gogh, a Stravinsky—confronts one. The history of criticism in the several arts is stocked with instances of the lagging reception accorded the 'new' poetry, music, or drama. The customary abhorrence of deviations is the *bête noir* of the creative artist no less than of the practical sociologist or reformer. Familiarity dulls, unfamiliarity—unless human inertia is overcome—repels (a psychological truism).

Yet, curiously enough, once the inertia of perception is discounted or overcome, it is departure from, rather than conformity to, the accepted norms of composition and design which is potent to evoke the most dynamic aesthetic effect, and is the source of the highest artistic power. The so-called 'norms' (unities of time and space, simplifications of perspective, geometric formalizations of space, tonalities and modalities, key-relationships and harmonic sequences, metrical schemata) seem to serve genius principally as points of departure, *i.e.* spring-boards or standards from which to revolt. It is not the smooth, conventional academic workmanship of Field, Clementi, Jonson, Canova, Alma Tadema, which evoke our keenest aesthetic reactions, but the defiant, revolutionary violations of tradition by such men as Beethoven, Wagner, Whitman, Rodin. Nor is this a matter to be facilely pigeon-holed under the caption 'novelty' and dismissed.

In the light of the foregoing observations, one may hazard the prophecy that the Meier Art Test earlier referred to, however admirable in its aim and execution, and however useful within its limits, will never serve to discover the rising genius destined to forge a new aesthetic idiom. In its paired reproductions, the mutilated or altered member (in which some decorative canon, some maxim of balance or design or unity is violated) has often unintentionally become the more expressive and dynamic, yielding the keener aesthetic satisfaction. (One picks at random Plates 82, 88, 90, in illustration.) It is matter for chagrin that the psychologist should lag so far behind the great art and the progressive criticism of the twentieth century; busying himself with antiquated conventions and the tepid agreeable and disagreeable,[5] rather than setting out to explore the natural expressiveness of the various aesthetic factors (line,

[5]This is perhaps not quite fair to the Meier Test, since in certain instances emotional congruence or incongruence rather than the violation of some formal maxim may form the basis of selection.

form, color, tone quality, etc.), and the degree of universality with which they may evoke in perception the whole gamut of feeling (exciting, soothing, exalting, depressing).[6]

The grounding of aesthetic science upon universal norms or values (derived either from social sanction or the eternal verities) boasts, it is true, a long and venerable history; running back from Fechner through Schopenhauer and Kant to Plato and the Idea. Classicists and academicians in every age have, moreover, lent their energies to exalting into a cult and ritual the formulae for the correct outline of drama and symphony, or the laws of balance and symmetry in plastic art. Throughout the nineteenth century the quest went eagerly forward for the 'golden section,' the perfect proportion (*vide* Fechner's informal calling-card questionary as to the fitting dimensions for the rectangle), or the rules of substitutional symmetry.

Blinded by the hedonistic fallacy (the confusion of the aesthetic with the agreeable) to the true problems of aesthetics, investigators have spent themselves upon the assembling of innumerable and futile statistics on the pleasantness and unpleasantness of colors, color combinations, consonances and dissonances, harmonic sequences, lines and forms, etc. Such researches, however, have resulted neither in the setting up of canons of good taste, expedient for the education of the connoisseur, nor in the establishment of principles serviceable for the guidance of the artist in the manipulation of his media. The total upshot of so much misdirected energy is a mere handful of tentative norms of the crudely offensive or inoffensive; useful at most to the advertiser or lesser craftsman as honey-guides to syrupy stuff suitable to lure the prospective buyer, or as danger-signals warning of dynamic matter, to be handled with impunity only by a genius. Their significance for Aesthetics proper is, however, a vanishing quantity. What possible light can the charting of the relative pleasantness or unpleasantness of bi-tones throw on the charm of a tonal picture by Ravel? Or what is the relevance of Landis's projected norms of facial beauty or of design to the heightened effectiveness of the outlandish landscapes of Van Gogh or Ryder, or to Mestrovic's unrealistic sculptured portraits?

Even as contributions to 'pure science' these statistical studies of the agreeableness of simple sense-impressions and their patterns are unimpressive. Experimenters frequently admit themselves baffled. Even within limited and selected groups the expected universality of appeal is lacking. Subjects either display shocking idiosyncrasies, or manifest a perverse interest in the imperfect or slightly off-color. Such trends as are discoverable find little warrant or substantiation in the great and effective art of today—or yesterday. Gestaltikers seize the opportunity to drive home the dogma of the swamping of the part (plus its affective tone) in the whole. The possibility of an analytic science—or any science—of Aesthetics recedes into the distance.

Now in the study of attitudes cited earlier, it is only fair to add, Anderson obviously struggles to avoid this second fallacy, the confusion of the aesthetic with the affective. Misled, however, by the superior 'richness' of perception occasionally reported by his observers (and passing up, in my opinion, some

[6]Pioneering work along these lines has already been undertaken by Vernon Lee and Lundholm (in design and line), by Wells and by Bullough (in color).

excellent leads as to the peculiar and *personal* character of the experience), he reads the matter of norms, of approval and disapproval, and satisfaction based on the recognition of formal elements into the record (and into his subsequent instructions). In thus identifying the aesthetic attitude with the critical, and tracing the secret of aesthetic charm to the cognition of abstractions, he is of course not without precedent and support in contemporary criticism. The formal elements are indeed the A B C of art; and in every era small esoteric groups are extant who proclaim the identity of the aesthetic with intellectual pleasure in the play of form,[7] viewing Art itself as an abstraction, couched in an idiom fully intelligible only to the elect. The more impersonal and non-representative this idiom, the more purged of every hint of emotion or of the natural world of living objects, the more nearly it approaches to perfection (a view in many points germane to that of Kant, who like most metaphysicians warped aesthetics to his own ends, fabricating from it a bridge between the practical and the ideal, and reducing the aesthetic function to a kind of setting-up exercise for pure reason).

The bulk of liberal opinion, however, among both artists and critics, has always refused to hypostasize the purely formal or technical, or stress the intellectual at the expense of the emotional. The essence of art, in this maturer, saner judgment, is social expression, the heightening and broadening of experience, the 'summing up of life.' The formal elements are ancillary merely to this purpose, meaningless when abstracted from it. It is true that although subordinate, in this perspective, to the total expressive content, emotional and intellectual, the formal elements (such as rhythm, rhyme, decorative motif or color) appear on occasion autonomous, sheer embellishment, focussing attention in their own right. Even here, however, their independence, abstractness or intellectualization may be illusory merely; and, however masked, their vital articulation with the total schema the sole condition of their apotheosis from the affective to the aesthetic.

Manifold and diverse, it is true, are the auxiliary and secondary (pseudo-aesthetic) functions of the formal factors, which controlled psychological observation discloses. Sometimes they appear to serve a semi-hypnotic office, riveting attention on the aesthetic object, or absorbing and monopolizing the motor functions of the organism, so that the practical adjustive processes give way perforce to the aesthetic. Again, in art-works of a larger scope, they may operate as a concession to the limited span of apprehension, binding the discrete parts into a whole, and furthering unification and order (as *e.g.* in the dominance of the diagonal or the vertical in a mural painting, or of the ogive in the façade of a cathedral).[8] Or the rôle of formalization may be to heighten, by its very artifice, that sense of mastery of the craftsman over his media, of the spectator over his emotions and ideas, without which aesthetic exhilaration cannot reach its ultimate pitch. In many cases, emphasis on pattern and conventionalization may perform yet another subsidiary function, the 'distancing' of matter too poignant in its naked realism for aesthetic use (the

[7]Inclusive of such personalities as Hanslick, Clive Bell, Hildebrand, C. K. Ogden—mainly of the 'Art for Art's sake' type.

[8]Hence facilitating the heightening of effect by promoting compactness, the presenting of *multum in parvo*.

Aristotelian catharsis of emotion). Yet in the last analysis the formal elements achieve legitimate aesthetic status solely by virtue of their intrinsic (or mimetic) symbolism, their germaneness to the dominant mood or emotion or idea of novel, symphony, landscape, portrait. Their final aesthetic justification lies in their character as vehicles of feeling (via movement), rather than in any intellectualizing moment (however masked this truth may be from the vision of the unpsychological observer).

The exploiting of this aspect of aesthetic process—the range of feeling absorbed from everyday emotional experience by the various rhythms, timbres, colors, melodic phrases, tonalities, lines and figures—is unquestionably the feasible and urgent problem of present-day aesthetics. The norms for expressiveness, though taking shape only as rather fluid generalizations, dependent upon objective setting and the subject's tuning, will yet, if preliminary observations are to be trusted, possess more validity and generality than those erected upon that most subjective and variable of judgments, the pleasantness-unpleasantness alternative. In any case, a moderate array of observations of the type proposed, carried on under various conditions of isolation and of incorporation in art-works in which the element studied dominates, will be found to promote more active and imaginative aesthetic appreciation in the subject than hundreds of judgments on the relative pleasantness and unpleasantness of the various sense-qualities. It is here that the silhouetted profiles of White and Landis should prove of value, as the various sets of emotional photos already have, in trying out the emotional suggestiveness of various facial configurations. Subjects will be found to differ widely in their responsiveness, although a vast mass of plastic art presupposes a universal sensitiveness and a fair agreement.

Hand in hand with researches of this order should proceed the study of the aesthetic function *in toto* (wrongly characterized, I believe, by Anderson as *impersonal*—as indeed the reports of many of his own subjects indicate). Exploration of the conditions that promote that peculiar blend of the proprioceptive and the teleceptive which marks the aesthetic moment[9] should open up new ground in the related province of 'creative imagination.'

In conclusion, it cannot be too vehemently urged that in the project in aesthetics here outlined the immediate emotional toning rather than the accessory 'associations' or imagery should constitute the focus of attack. Given the aesthetic *Einstellung* or *Anlage*, this emotional toning is as immediate and direct as the visual or auditory matrix of the perception, embuing it, fusing or amalgamated with it. Hints of this often slighted or neglected truth (of which, however, a few *Verstehen* psychologists and *Gestaltiker* appear to have taken cognizance) crop up throughout the reports of Anderson's observers. Here the developing graphic methods of psychophysiology must ultimately be relied upon to supplement and substantiate the pioneering work of descriptive psychology.

Cornell University ELSIE MURRAY

[9]Sometimes inadequately designated as 'empathy,' or the 'identification of the subject with the object,' or, better, by Piaget's infelicitous 'indissociation.'

Heterogeneity and the Theory of Factors

Amid much controversy which has been only of the 'barking in' type, it is pleasant to encounter such a genuine and fair attempt to advance science as the present work of Cureton and Dunlap.[1] In keeping with the rest of it, they have courteously sent me a copy of their manuscript and have invited me to add my own comments to their publication.

Well, the gist of the work, I think, is that I had suggested that "heterogeneity may give rise to spurious group factors which may tend to obscure the general factor." Whereas they quote Kelley as arriving at the opposite conclusion, namely that "heterogeneity should spuriously increase the relative importance of the general factor." They end by concluding that "heterogeneity increases the relative importance of group factors," which obviously agrees with the view credited to myself. In support of their conclusions they bring a formidable array of evidence; tests carried out on many hundreds of persons, belonging to nations that seem to have been nearly as numerous; pages and pages of toilsome and complicated mathematics. But I venture to think that the matter can be illuminated by a very brief consideration *a priori*.

Let a and b denote any two variables, whilst g, x, and s are respectively a general factor, a group one, and a specific one; h is the additional factor derived from the character in which the subjects are heterogeneous. For the convenience of avoiding roots, let us suppose that $\sigma_{s_a} = \sigma_{s_b}$ and that $r_{s_a h} = r_{s_b h}$. We get then at once[2]

$$r_{(g+x+s_a+h)\,(g+x+s_b+h)} = \frac{(\sigma^2_g+\sigma^2_x+\sigma^2_h)+(2r_{gh}\sigma_g\sigma_h+2r_{xh}\sigma_x\sigma_h+2r_{sh}\sigma_s\sigma_h)}{(\sigma^2_g+\sigma^2_x+\sigma^2_s+\sigma^2_h)+(2r_{gh}\sigma_g\sigma_h+2r_{xh}\sigma_x\sigma_h+{}^2r_{sh}\sigma_s\sigma_h)}.$$

Now, by mere inspection of this equation, we see that *any* of the three kinds of factors involved, general, group, or specific, can acquire increased importance by its correlation with h. In the case of the present work some correlation between h and g would be expected by all who are not fanatics for racial equality; whilst some between h and x was as good as certain, owing to the verbal factor. Which should actually predominate is a mere accident depending on the construction of the tests in detail.

More serious than this point, however, seems to be another one, about which these authors and myself are perhaps less well agreed. In common with most psychologists and statisticians, they appear to treat heterogeneity as being essentially a source of statistical error—comparable in this respect with "attenuation"—so that it should always and everywhere be so far as possible eliminated. In particular, they represent heterogeneity of age, sex, race, and education as newly discovered sources of an illusory appearance of a general factor. In this sense they write approvingly of the "penetrating observation" that the general factor may be "principally composed of verbal elements and heterogeneity."

Against all this, I would urge historically that the influence of such heterogeneity of age, sex, etc. is not anything newly discovered, but on the contrary

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[1] E. E. Cureton and J. W. Dunlap, Some effects of heterogeneity on the theory of factors, this Journal, 42, 1930, 608-620.

[2] See C. Spearman, Correlations of sums or differences, *Brit. J. Psychol.*, 5, 1913, 417-426.

constituted the larger portion of the original work on the general factor.[3] In this work, as ever afterwards, the view was repeatedly stressed that the effect of heterogeneity is by no means spurious essentially, but only when, and in so far as, it happens to be irrelevant to the problem at issue. Thus in the very same situation, the same heterogeneity may be irrelevant and undesirable for one problem, but the reverse for another. For example, we may at one time only wish to take account of the individual differences that exist within a single race, whereas at another time we may have to bring into the reckoning the augmented differences that come from putting several races together. Here, as elsewhere, we may see that the effect of heterogeneity is not any statistical illusion, but a reality in the course of events. As for regarding it as something that should always be eliminated, I would say that *every* individual difference —whether of *g*, *x*, *s*, *h*, or otherwise—is *wholly* "composed of heterogeneity" (though I have little liking for the phrase). If you really exclude all heterogeneity, then all the differences, your very universe of discourse, incontinently vanish!

University of London C. SPEARMAN

AN UNCONSIDERED SOURCE OF MATERIAL FOR THE PROBLEM OF INDIVIDUAL DIFFERENCES

At a Conference on Individual Differences held in Washington in May, 1930, the author expressed the opinion that a profitable source for the study of individual differences was relatively untouched. This consists in a gathering together of the individual results of the studies in experimental psychology which were performed under strictly comparable procedures. Such a body of experiments exist for a great many psychological processes in a magnitude

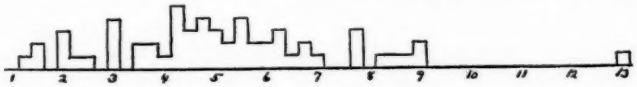


FIG. 1. DISTRIBUTION OF THE DIFFERENCE THRESHOLDS FOR LIFTED-WEIGHTS

sufficient to give the results statistical validity. Although these experiments were performed with a primary interest in a study of the central tendency, a treatment such as that here suggested must yield results of interest to differential psychology.

In order to illustrate the point, there are here gathered together the results of some 58 *Os* in the perception of lifted weights done by the method of constant stimuli in various studies by Urban, Rudisill and the author, under exactly comparable experimental procedures and instructions and with a standard weight of 100 grams. The distributions for the interval of uncertainty (which

[3] Cf. C. Spearman, The proof and measurement of association between two things, this JOURNAL, 15, 1904, 72-101; "General intelligence" objectively determined and measured, *idem.*, 201-293; also F. Krueger and C. Spearman, Die Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten, *Zsch. f. Psychol.*, 44, 1907, 50-114.

may be considered as the measure of sensitivity of the *O*) and for the point of subjective equality will be found in the accompanying figures (Figs. 1 and 2). The measures of central tendency for these results as given in terms of the average are:

Average Interval of Uncertainty = 5.11, P.E. 1.49
Average Point of Subjective Equality = 96.47, P.E. 1.03

In all of these studies the space error was eliminated by a turning-top table and the time errors were all in the first order. It is of interest to note that there is much less variability of the effect of the time error (as measured by the point

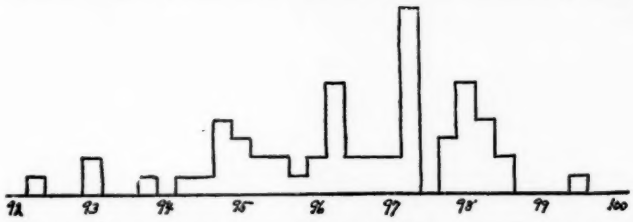


FIG. 2. DISTRIBUTION OF THE POINTS OF SUBJECTIVE EQUALITY FOR LIFTED-WEIGHTS

of subjective equality) than of the sensitivity of the observers (as measured by the interval of uncertainty). The point of subjective equality is dependent upon the relative number of greater and less judgments and the interval of uncertainty is dependent upon the relative number of equality judgments. One may therefore conclude that there is greater individual variability for the equality than for the extreme judgments.

The present note is presented in an effort to indicate what may be done for the problem of individual differences by a systematic study of the experimental literature. It is also published in an effort to urge authors of experimental studies to publish all of their individual results so that these may be of use for future studies.

University of Pennsylvania SAMUEL W. FERNBERGER

APPARENT MOVEMENT IN LISSAJOU FIGURES

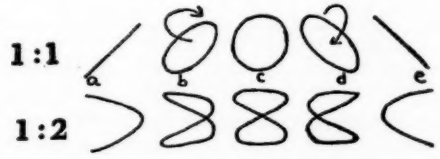
Lissajou figures, so named in honor of the investigator who first studied them, are figures which arise from the composition of harmonic movements. Since tuning forks give a quite clear simple harmonic motion, it is possible to secure interesting patterns by reflecting the combined movements of two forks upon a screen.[1] Physicists use these figures in making very exact determinations of the vibration rate of a fork. If the d.v. of a standard fork is given, the d.v. of an unknown fork may be deduced from the figure yielded by the two forks

[1]I am indebted to my colleague, Dr. P. H. Gaehr, for my first acquaintance with Lissajou figures. They can be produced by electrical means as well as by tuning forks. The Westinghouse tube oscillograph is available for the purpose, but the set-up involves niceties that may well discourage all but experienced physicists.

acting in unison. These figures, however, have a special significance for the psychologist in view of the current interest in apparent movement. The pattern of the Lissajou figure depends upon the ratio of the two vibration rates used to produce them; but I have found no printed reference to the fact that such figures execute very lively movements that have all of the familiar signs of illusory motion.

Two electrically driven forks are required, whose frequencies are adjustable. At the end of one prong of each fork, a small mirror is secured. One fork is set up to vibrate horizontally, the other fork to vibrate vertically. They are so placed, relative to each other, that a focussed beam of light strikes one mirror, is reflected to the other mirror, and then on a screen. A silver screen gives the best results.

The beam of light traverses its path on the screen so rapidly that the observer sees the path, but not the motion of the beam. If one should attach a light pendulum to the bob of a heavy pendulum, and set both bobs to swinging at right angles to each other, one would have an approximate, slowly moving



model of what happens during the formation of Lissajou figures. The type of pattern yielded by the tuning forks depends on the ratio of the two d.v. employed. In the accompanying diagram, the reader will find the figures that accompany two simple vibration ratios. If the ratio happens to be exactly like one of those entered on the left margin, one gets one of the figures in that row. If the ratio departs slightly from perfection, the screen shows a series of patterns in succession, from left to right and then from right to left. This is the situation that interests the psychologist. In place of seeing one figure being replaced by another, the observer may see *one* figure rotating, presenting its different 'sides' to the observer.

The writer, while planning a more extensive study of these figures, may report some general results of personal study of the Lissajou figures.[2] The most general statement that one could make is perhaps the remark that Lissajou movement has both subjective and objective determinants. We may note

[2]A few suggestions may be made in the interest of others who wish to explore the possibilities of these figures for their psychological significance. One should have forks whose weights can be adjusted automatically while the forks are in motion. The possibilities of research will also be greatly increased by using forks whose magnets are on sliding bars, which can also be adjusted while the fork is in motion. I can recommend Lissajou figures as excellent objects of research considered from the observer's point of view. The figures are unique, the apparent movement is very realistic, much more so than the familiar demonstrations of phi, delta, gamma and other movements. Observations can easily be taken from a score of observers at the same sitting.

some of the subjective factors first. Consider, for instance, the tri-dimensional movements yielded by the figure arising from unity ratio, the ratio, 1 : 1. To begin with, the observer may not notice the tri-dimensional motion at all. *Attitude* thus emerges as a familiar determinant. If motion appears, one sees the line a as an ellipse on edge. It arrives at stage b by rotating (see arrows), at the same time altering its vertical axis so that the observer finds himself facing a circle at stage c. Still rotating, the vertical axis now swings to the opposite direction giving the ellipse d. Soon, the edges of d are again in the line of regard, giving the line e. These movements may, however, be entirely reversed, by that same mysterious change of 'mental set' which changes the familiar reversible perspective figure. What is more, it is easy to compel the figure to rotate in clockwise manner to stage c, pause, and continue the movement in counter-clockwise direction.

The subjective factors also appear in the familiar garb of *habituation*. For instance, the clockwise motion is easier to produce than the reverse. Continuous motion, whether clockwise or the reverse, is easier to secure than an alternation of the two. If, while trying to impose counter-clockwise motion, the figures suddenly become larger or rotate more rapidly, the observer finds his control 'slipping' and the figure moves in the easier clockwise direction. The loss of control is very much like the loss of coördination that would appear if a weight one handles suddenly decreases. A little practice is required to secure any form of motion. I find, as Köhler noted with a similar phenomenon with a static figure,[3] that if the figures take a new orientation on the screen (secured by changing the plane of vibration of the forks while maintaining them at right angles to each other) that the effects of previous practice are partially or wholly lost, and one must renew practice in order to recapture the motion. Past experience, indeed, reaches deeply into these phenomena. The physicist, with his entire attention fixed on the *form* of the figure may fail to see the tri-dimensional motion; but he is bound to see motion on the plane of the screen. He sees a line (a) splitting to form the ellipse (b), etc. But why this result of *one figure changing* instead of *five figures in succession*? Perhaps it is past experience with material bodies which leads him to see, not many bodies successive in time, but one body moving. But, since this inveterate habit rests upon a truth regarding physical bodies, *i.e.* the truth that many forms appearing at the same point in space, are but *aspects* of one body moving, perhaps this determinant may be regarded as objective.

There are of course objective determinants of the motion. The form of the figure, the speed of motion, the size of the figure depend on the apparatus. One cannot impose *any* motion upon a figure. It appears that the apparatus determines several possible motions from which the observer may choose.

The primary indication of these results is to contradict the view of illusory motion which seeks its complete determination in objective terms, leaving out of account such factors as the direction of attention, instruction, attitude. Within limits, one sees what one *intends* to see.

Wells College C. O. Weber

[3]W. Köhler, *Gestalt Psychology*, 1929, 186.

The Fifth Annual Meeting of the Midwestern Psychological Association

The meetings were held this year at Antioch College, Yellow Springs, Ohio, on May 23 and 24. There were five programs of papers and a round-table discussion. The customary reports from the laboratories were given in the afternoon of the last day, which was followed by the annual dinner in the college dining room.

The first session began with two parallel programs. Program I, presided over by Dr. C. A. Ruckmick (Iowa), opened with a contribution from J. A. McGeoch (Arkansas) on the influence of different interpolated activities upon retention. The results of Müller and Pilzecker were apparently contradicted in that relatively small amounts of inhibition were discovered when three different sorts of activities were interpolated. These three activities consisted of the following: (1) ten repetitions of a second list of nonsense syllables followed by reading for the remainder of the interval, (2) continuous tapping, (3) ten electric shocks administered at irregular intervals while the subjects read humorous material. In another series continuous color-naming was used in place of the electric shocks. Tapping gave the least inhibition.

T. Perkins (Kansas) showed a number of diagrams depicting the tendency towards symmetry in visual memory. This illustrated the so-called *law of pregnancy*, namely that the configuration tends to become as stable or unified as possible. In the discussion it developed that the tendency was in the direction of meaningful material when the original presentation was relatively devoid of meaning.

The rôle of head movements in auditory localization was analyzed by P. T. Young (Illinois). When the movement of the head becomes ineffective by extending the pinnae of the ears, by means of tubes, beyond the walls of the *O*'s room, sound localization is restricted to the semicircular arc outside of the visual field, generally in the direction of the occipital region. The tendency was toward limiting localization to a plane rather than to a tridimensional manifold when head movements were no longer operative.

H. Woodrow (Illinois) read a paper on individual differences in the reproduction of temporal intervals. In an extended study no 'indifference' interval was revealed below which intervals would be overestimated and above which they would be underestimated. The results indicated that variation in the instruction given to *O*s, or different attitudes tacitly assumed by *O*s, accounted for the constant errors in judgment.

M. A. Weichbrodt (Northwestern) compared three different methods of transmitting speech: lip-reading, vibrational stimulation of the fingers with the microphone before the mouth, and the same procedure with the microphone held against the larynx. The stimuli were consonants followed by the long vowel 'e'. In general, lip-reading was slightly more efficient although there were certain combinations which gave best results when the microphone was held against the larynx. Simultaneous visual and tactual presentation, however, brought still better results. Artificial deafness was produced in some cases by means of four telephone buzzers.

Program II, Dr. J. P. Porter (Ohio) presiding, opened with a paper by S. Renshaw (Ohio State). The results of a two-year investigation of the influence of motion pictures on children's sleep gave conclusive evidence that children

exposed to romantic situations in motion pictures were much more disturbed by restlessness during their sleeping period than other children not so exposed.

M. G. Cutsworth (Kansas) studied the successive brightness discrimination of chicks and found that 85% of each group learned the problem completely, making choices in terms of the relations of the total brightness-situation rather than in terms of isolated elements and absolute brightnesses.

W. Beasley (Ohio State) presented the results of work done over a period of several years on the monaural phase effect with pure binary consonances. He has perfected a very ingenious apparatus by means of which a composite tone containing five partials can be produced with the possibility of making independent changes in phase.

D. McL. Purdy (Kansas) studied the differential chromatic thresholds of the spectral colors. The threshold of a hue was found to be inversely proportional to its complementation-valence whereas the chromatic thresholds of two complementary hues were in the same ratio as the respective intensities of these hues that were required for complementation. This effect is more easily explained in terms of some theory like the Hering theory of antagonistic processes than in terms of the Young-Helmholtzian theory.

Some aspects of the psychophysics of the vibrato were presented by J. Tiffin (Iowa) using the psychometric procedure of the constant process. It was found that the vibrato at threshold frequencies was practically independent of the rate of the vibrato and independent of the scale region. The pitch of the frequency vibrato was localized as definitely as was the pitch of a tone of constant frequency; when the vibrato contained a wide extent of frequency, however, some *Os* localized the pitch extremely sharp and others extremely flat.

H. R. DeSilva (Kansas) through an interesting technique has studied the 'blindness' of the so-called blind-spot discovering a number of anomalies.

Program III, Dr. R. H. Wheeler (Kansas) presiding, was held Friday evening and had an attendance of over two hundred people. H. Klüver (Behavior Res. Fund, Chicago) showed in terms of motion pictures some of the functions involved in the 'relational' reactions of monkeys to sensory stimuli. The behavior of the animals while learning to compare weights, together with additional cues from the visual and acoustic field, was clearly demonstrated.

The next paper by L. S. Tsai (Chicago) was also demonstrated by motion pictures. After a theoretical analysis of handedness, typical cases of left-handedness, right-handedness, and ambidexterity, selected from among two hundred white rats, were illustrated.

H. M. Johnson (Mellon Inst.) showed some very interesting photographs indicating various postures assumed during sleep throughout the night. These pictures were automatically taken whenever posture radically changed. A great variety were shown indicating that there is no one favorite way of sleeping.

D. F. Boder (Lewis Inst.) demonstrated the ability of white rats to react to lights which were flashing at different speeds. Some of the animals were also taught to discriminate between a continuous light and a flashing light. A demonstration of the apparatus was given.

The last paper on this program by J. R. Patrick (Ohio) concluded that white rats do not learn the maze pattern alone but only in connection with other incidental stimuli acting at the same time. When there is a sudden

shift of incidental stimuli numerous errors are made in succeeding test-runs. These results were related to the problem of *redintegration* and of *Gestalt*.

On Saturday there were again two parallel programs. Program IV, Dr. W. S. Book (Indiana) presiding, began with a paper by N. W. Shock (Chicago) on the acid-base balance of the blood and mental performance. By means of the micro-chemical technique, the percentage of red cells, the pH, and the total carbon-dioxide content obtained from a single sample of two drops of blood, 15 *Ss* were found to have a slight, though consistent, positive correlation between an alkaline blood reaction and the report of well-being. There were no significant correlations, beetween blood measures and mental performance.

K. S. Lashley (Chicago) reported on cerebral function in pattern-vision of the rat. The rats were required to jump a short distance at a visual pattern. The correct pattern then dropped away and food was supplied. This was used as the measure of visual acuity. When the occipital cortex of the cerebrum was removed patterns were no longer distinguished but single objects, small differences in brightness and, to some extent, distance were still distinguished. Smaller lesions within the visual area retarded the formation of visual habits.

S. H. Bartley (Kansas) reported some preliminary work on action-currents of the brain.

L. E. Wiley (Chicago) read a report concerning an auditory habit and its cerebral mechanism in sixty albino and hooded rats. After the entire cortex had been destroyed forty-nine animals that survived were taught to relearn the T-maze with the same negative response to auditory stimuli as before. The brains were then histologically examined with the result that the part of the cortex involved was fairly well determined but within this part no one portion seems to be more essential than any other.

The next paper by A. R. Lauer (Ohio) furnished miscellaneous information concerning electrical resistance in living organisms as related to age, sex, skin color, and various other physiological conditions. Resistance appeared to be relatively constant from time to time in the same individual with higher values in women than in men, and much higher values in sleep than in the waking period, and with some racial differences.

M. L. Steckel (Chicago) reported on the relation between parental age and the intelligence of offspring. With maternal ages ranging from fifteen to fifty-one years and paternal ages from fourteen to seventy-one and taking account also of the amount of disparity between the parental ages it was found that on the average children were more intelligent when the age of the mother was twenty-six or over, and that of the father, thirty or over, than below these respective age limits. The nearer the ages of both parents approached each other the higher was the intelligence rating of the children apt to be but it was also favored if the age of the father was in advance of that of the mother.

Program V, Dr. J. J. B. Morgan (Northwestern) presiding, began with a study of personality by A. Jersild (Wisconsin) in which the various factors were itemized and evaluated.

L. L. Thurstone (Chicago) proposed the 'method of absolute rank order,' also called the 'order of merit method,' as one of the more promising of psychophysical procedures as was shown by plotting the relative frequencies. Very small discrepancies were obtained between the much more laborious form

of the constant method, known as the method of paired comparisons, and this much simpler method of rank order.

E. L. Schott (Henry Ford Hospital) investigated the superior intelligence of patients with nervous and mental illnesses. Data collected over a period of four years showed no causal relationship between intelligence and instability.

The relation between ocular dominance and handedness among psychopathic cases were studied by N. B. Cuff (E. Ky. St. T. C.). By means of a variety of tests given to 203 children in grades two to eight, the speaker pointed out that both eyedness and handedness were very broad in their significance and showed minor variations in each individual. From 20-30% of the children were probably natively left-handed, while 93.6% were at the time of the study ultimately right-handed.

A new objective test by introversion-extraversion was proposed by A. R. Gilliland (Northwestern). These traits were defined in terms of their extremes so that out of 125 cases only three were found to be overlapping. Tentative norms for both normal and abnormal groups were shown.

H. H. Anderson (Iowa) reported on the incidence of behavior problems and personality differences in school. A number of forms were prepared in order to determine the various factors relating to maladjustments.

F. Fearing (Northwestern) presented some interesting photographs of the postural expression of emotion made by professional dancers. Preliminary results showed the part played by face, trunk, arms and legs as a cue to emotional expression, with both expert and non-expert judges.

About twenty individuals showed a considerable interest in the round-table conducted by Dr. R. H. Wheeler (Kansas) on the contribution of Gestalt to the problem of learning, which ran simultaneously with the other two programs. During the lunch hour, which followed these programs, about fifty members and friends of the new honorary psychological fraternity gathered at the first district convention. Dr. C. A. Ruckmick (Iowa) spoke briefly and encouragingly on "The Hope of an Honorary Psychological Fraternity." Officers for the ensuing year were elected.

On Saturday afternoon general laboratory reports were called for with Dr. W. B. Pillsbury presiding. A number of these reports were available in mimeographed form. Among the laboratories represented were Antioch, Arkansas, Chicago, College of the City of Detroit, Illinois, Iowa, Inst. for Juvenile Res., Kansas, Kentucky, Lewis Inst., Michigan, Missouri, Northwestern, Ohio State, Ohio University, Ohio Wesleyan, and Wisconsin.

This was followed by a business meeting at which various official reports were read and the new proposed constitution was adopted. All advanced students of psychology now become eligible for membership. The officers are to be elected by mail ballot. A number of other changes were also made. The invitation of the University of Chicago to meet there next year was accepted.

At the annual dinner the president of the Association, Dr. M. F. Meyer (Missouri), read a carefully prepared paper on "The Religion of Adam and Eve." The Biblical story was interpreted as being no 'sex story' at all but a beautiful piece of poetry showing the transformation of the passion of love into the realization of life with its duties and responsibilities to society and the human race.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

A History of Experimental Psychology. By EDWIN G. BORING. New York, Century Co., 1929. Pp. xvi, 699.

In terms of comprehensive scholarship well-seasoned and integrated no other work has appeared in America equal to this history since Titchener's four-volume series of manuals in experimental psychology. Although this may seem like a headline to call the reader's attention to what follows he very likely will find that there is adequate justification for it. The book begins with a broad foundation sunk deeply into the ground of other sciences and ultimately touching the basic work of Greek cosmology. The first chapter deals with the evolution of the scientific point of view and presents materials from before Aristotle to Darwin in support of the contention that science is fundamentally based on the method of observation. Rationalization is not foreign to science but comes legitimately only after observation and recording of natural phenomena have taken place on the basis of premises which "are given externally." The reviewer is not absolutely certain of the temporal sequence of these scientific processes but assumes that the author refers to the logical priority of the steps taken. Astronomy is the first science to fit into the requirements set down by the author and, therefore, the oldest science now in existence. This contrasts sharply with the reviewer's opinion that mathematics is usually referred to as the oldest science. It is not, of course, its principal task to observe natural phenomena but only to note abstract relationships between them. Some very clear expositions are given of early astronomical observations. Early physics and biology together with their gradual development to recent times are briefly reviewed. The much argued theory of emergent evolution is not discussed, however, possibly because the author does not consider its influence on experimental psychology important.

This chapter forms the introduction and serves, as does the overture to an opera, in suggesting a number of themes which are later much more extensively treated. Therefore such succinct statements as "physiology gave birth to experimental psychology" must be allowed to produce their shock of discord when one thinks of the foster parent of physics, because later the author takes considerable patience to clear up this situation. It is easy gun-play for the reviewer to note what he thinks are serious omissions but even considering the small space that is allotted to the early Greeks it is difficult to understand why the name of Alcmaeon, a younger contemporary of Pythagoras, was omitted, in view of his rather startling contributions to anatomy, physiology and medicine. This chapter ends as do all the chapters with a few pages of notes on each one of the major headings, citing references for further reading, and commenting on minor details suggested by the text. Here one misses, however, the enlightening influence of Gomperz and Windelband in favor of the more modern writers on the history of science.

The next chapter admirably summarizes the outstanding work done during the first half of the last century on the physiology of the nervous system. Going with disproportionate detail into Johannes Müller's *Handbuch*, it were better here and in the notes if the various *Abtheilungen* were repeatedly referred to as "parts." In the third chapter the mind-body problem finds its early development in phrenology, although due credit is given to special writers such as Descartes who bravely faced this task and made outstanding contributions to the subject. The more detailed physiology of the brain then follows together with the general histology of the nervous system. After a very competent discussion of the doctrine concerning the specific energies of nerves with particular emphasis on Müller and Helmholtz, we are carried through the early contributions with respect to the several sensory modalities. Chapter VII furnishes an interesting summary of the development of hypnosis under the subheadings of the outstanding men. Here, as before in the discussion of brain localization, the author takes occasion to point out that the front line of science sometimes moves too fast for all supporting units to follow. Sometimes, too, progress is made through what appears to be, at least at the moment, a partial retreat. Another early contribution to experimental psychology comes from questions arising in astronomy concerning the personal equation. This closes the section on physiological psychology of the first half of the last century.

Now comes the historical treatment of a further approach through philosophy involving in some places a repetition of many names of men already partially treated. Descartes, Leibnitz, Hobbes, Locke, Berkeley, Hume, Hartley, the Mills, Bain, Spencer, Herbart, and Lotze are given due credit through extended discussion for contributions to the background of experimental psychology. There is much here that might be questioned from the point of view of interpretation but, when outstanding scholars disagree, an historical psychologist is entitled to his own independent evaluation. As a matter of fact not much controversial material is included. It must be understood, of course, that other writers are referred to beside the names mentioned.

We are presently almost at the middle of the book, about to enter the next section through the door bearing the inscription "the founding of experimental psychology." Chapter XIII begins the section with a detailed summary of the history of psychophysics. It goes into the matter of Fechner's life and continues with an appraisal of the mathematical development of psychophysics which few physiologists in this country could give. We then pass on in the next chapter to Helmholtz' contributions as a physiologist, physicist, and psychologist. His contributions are very competently given under the headings of sense-physiology, empiricism, unconscious inference, perception, scientific observation, and the fundamental laws of mind.

When we come to Wundt we find one of the richest chapters of the book. Wundt's system together with the fundamentals which underlie it are presented in such a way that one can not miss his tremendous service in staking out a valid field for psychology. The conditioning clause must be added, however, that because Wundt stressed, especially in the early years, the necessity of controlled introspection he brought in the materials which later furnished tinder for the behaviorists. The author does well to limit himself to the barest essentials of the Wundtian doctrine all the while stressing the importance of

this prodigious writer and worker in the interest of establishing a tangible, experimental science of the mind. The temptation must have been great to dig deeply here, since Wundt's energy, particularly later in life, was also directed into fields like those of social psychology and logic where the experimental techniques were presumed but not used. Brentano, Stumpf, and G. E. Müller form the theme of the next chapter where the material is again chiefly biographically treated. The reviewer easily but perhaps unfairly suggests that Stumpf's contributions in the direction of primitive music, especially his excellent work on *Die Anfänge der Musik*, do not here receive adequate treatment.

The next chapter bears the heading "The 'New' Psychology." Ebbinghaus, Külpe, Mach, Avenarius, Watt, Ach, Messer, Bühler, Titchener, Preyer, Delboeuf, Lipps, Ziehen, Münsterberg, Kraepelin, Meumann and a number of others including the psychological physiologists are among those whose contributions are woven into this chapter. Titchener naturally receives more extended treatment. The keynote to Titchener's contribution is found in the statement that he aimed to have "psychology stand upon its own legs" instead of "bolstering it up with any artificial props." Much is made of his intellectual isolation and his frequent polemics. The psychology of act, content, and form is separately treated in the next chapter with extended references to Von Ehrenfels and Lipps and Meinong.

There is a separate chapter on British psychology with separate headings for systematic psychology, animal psychology, mental inheritance, statistical method, Galton as a psychologist, and experimental psychology. Much biographical material is here as elsewhere introduced and the change to a different traditional point of view is gradually made with considerable understanding. We then proceed to the pioneering work of American psychologists with extended discussion of William James, G. Stanley Hall, Ladd, Baldwin, Cattell and others. In the last category the reviewer misses reference to Seashore, whose contributions to experimental work through his manual of "Elementary Experiments in Psychology" published in 1908 must have had considerable influence in the development of experimental psychology in those institutions which lacked the means of obtaining expensive apparatus. The movements of functional psychology, mental tests, animal psychology and physiological psychology form the ground work of the next chapter, which is followed by a chapter on *Gestalt* and behaviorism. The materials of both points of view are squarely presented. There is less negative criticism of the former than of the latter but the reviewer is surprised that the approach of Max Meyer receives no recognition here.

There are then two summaries, a recapitulation from Fechner to the present and a brief reinterpretation. The author bares his own attitude in frankly answering the question, to what extent has the new psychology justified itself? Experimental psychology is eminently worth while. "The person who doubts that the results have justified the importance that has been attached to the invention of experimental psychology must be either ignorant or influenced by a disappointment that the progress of experimental psychology has not aided in the solution of his own particular problems." The author's frankness is shown by his admission of disappointment that there has not been any great

idea or discovery that has revitalized the science, for which he gives two reasons: (1) It has had no great psychologists of the order of Helmholtz or Darwin whose contributions lie entirely in the field of psychology; (2) there has been considerable conflict within the camp of psychology itself. He believes that psychology should entirely relinquish "its philosophical heritage"—with which the reviewer heartily agrees, in so far as any science can so do. Curiously enough, one of the older sciences, physics, is now showing us an example in the opposite direction! There is a brief appendix on French Psychology and Psychology in Other Nations. The author is rather strict in his delimitations since he does not believe that France has ever been "at the core of experimental psychology." Comprehensive indices of names and subjects close the book.

The reviewer has found no occasion for retracting the statement made at the beginning of this review. Only those who know the amount of effort and discipline that lies behind historical contributions can appreciate the place which this book is bound to occupy in American psychology. Unfortunately too much material has been published in book form which lacks or fails even to approach the insight displayed in this volume. The style is lucid, direct, and lively. Interpretations, both expressed as actual textual content and implied in terms of place and area of treatment, may rightly differ from writer to writer. But there is no book now available that is as circumspect and pithy as this *History of Experimental Psychology*. Following the fashion of many modern books where such devices are found extremely useful, as in Count Luckner's *Exploits*, the inside covers and fly-leaves present a map showing the location of the more important universities in Germany, Switzerland, Austria, and Poland. The volume appears in an attractive format, the contrast of whose binding of gold on black may be significant: gold for the precious experimental material which gives psychology a right to exist; black to voice the disappointment of the author in regard to the mournful conflict within the tribes and the dearth of great *patres familiarum* in their midst.

University of Iowa CHRISTIAN A. RUCKMICK

Gestalt Psychology. By WOLFGANG KÖHLER. New York, Horace Liveright, 1929. Pp. x, 403.

In this book we have the long awaited *credo* in English of a leading member of the *Gestalt* school. While many may have expected Köhler's exposition to contain a definitive statement of the *Gestalttheoretiker*, the whole tenor of the book is against a closed systematic formulation of beliefs. The gaps and *terra incognita* of psychology are so great even for *Gestalttheorie* that Köhler speaks of the point of view as resembling "in 1928 a promising start more than a complete achievement." He says about his own efforts in this book that "nothing is easier than to criticize it." True, this book may be "the incomplete portrait of an incomplete thing;" nevertheless *Gestalt* psychology, born in 1912 at the latest, is as old as behaviorism, which Köhler treats as quite grown-up. A psychological system will be evaluated by its contemporaries before its authors feel it has reached its majority. Certainly, whatever Gestalt psychology may become, it is bound to be taken now for what it is now, and, with an exponent like Köhler whose penchant for theorizing is well known, no fears

need be felt for the position of the school nor its present expression in this book. It would be hard to find a more beautiful, clear exposition of fundamentals in the literature of any movement. Would that every school had as able a defender and as sure a position to defend!

Perhaps the dominant note of *Gestalt Psychology* is set by the question: "How shall we explain psychological phenomena?" The answer to this question takes us from a consideration of scientific method through the postulates and practices of behaviorism and introspective psychology to the point of view advocated by Köhler. The starting point of all science, according to Köhler, is and must be the world as it is found naïvely and uncritically, even though the naïveté may be lost as we proceed. In their beginnings all sciences start with qualitative facts before applying the more refined methods of measurement and analysis that characterize an adult science. Psychology is especially in need of a more naïve approach to its subject-matter since it is "in most cases easier to see 'anger' directly in a subject's attitude than to state and measure the adrenin in his blood" (p. 41). Measurement in physics has been used in the service of discovering the *nature* of the states and processes investigated but in psychology we have not been in the habit of asking questions about the processes through which a real knowledge of phenomena is gained (p. 49). Both the intelligence tests and the work with animals have failed, often, to reach to the bottom of what was being studied by neglecting to ask the right sort of question.

The attempt of the behaviorist to imitate adult physics in the young science of psychology our author regards as abortive and a failure. It has led to a fundamentalism in outlook which has delimited the field of psychology too narrowly, especially where it concerns the rejection of so-called subjective phenomena, or, more properly speaking, the facts of direct experience. "The behaviorist must be wrong when he declares that . . .direct experience when observed introspectively is not being observed scientifically, since in the case of observation in physics the situation is similar: the material to be observed and the process of observing belong to the same system" (pp. 30 f.). Dichotomizing the world into 'subjective' and 'objective' is no longer possible since all observations depend upon processes in the same system, viz., my physical organism (p. 30). Some psychologies bear the marks of 'subjectivity' and others of 'objectivity' but the observation of both is essentially the same. The contrast between them is something different from the genetic 'subjectivity' of both types of process or observational material. It might be inferred from this criticism that Köhler is a subjectivist or a sort of introspectionist. Nothing could be farther from the truth. He has just as little sympathy for the position of the introspectionist, whose psychological world is quite foreign to him. "When I talk about a chair, the chair of my everyday life is meant and not some 'subjective phenomenon' to be observed by highly trained introspectionists, but utterly unknown to me" (p. 25). Introspection, like behaviorism, approaches its data with a highly sophisticated (or shall we say prejudiced?) attitude which Köhler believes is blinding us to many interesting problems.

Introspectionists and behaviorists are essentially alike in their logical approach to their problems. Both have erred in employing what the reviewer would call 'direct' or 'simple' analysis. Köhler does not mince words in stating

what he thinks about the accomplishments of either school. Of introspectionism he says: "It was strange to see how the description of a direct experience, what happens, for instance, during a single comparison of two tones or colors, could fill hundreds of pages without giving us the slightest positive idea about how the occurrence and accuracy of such a comparison might be explained" (p. 10). Of behaviorism: "As an imitation of physics it is scarcely a satisfactory achievement for the behaviorists to have taken the old concept of reflex action from physiology (including the reflexes of inner secretion) and to give us no further comprehension into the formation of new individual behavior than is offered by the concept of positive and negative 'conditioning' " (p. 55). Finally, both schools, in spite of superficial differences, are really of the same breed, for "I do not see why introspectionism should be preferred to behaviorism or vice versa. They are so much alike in their fundamental attitudes, that all their wrangling seems like a family quarrel to the onlooker" (pp. 101 f).

The alternative to the logic of behaviorism and introspection is the fundamental thesis of *Gestalt* psychology. It asserts that *"to a context, experienced as 'one thing' belonging together, there corresponds a dynamical unit or whole in the underlying physiological process"* (p. 106). Stated a little differently, "instead of reacting to local stimuli by locally and mutually independent events, the organism reacts to an actual *constellation* of stimuli by a total process which, as a functional whole, is its response to the whole situation" (p. 106). The importance of this principle lies in the fact that "this is the only viewpoint which can explain how to a given local stimulus there may correspond altogether different experiences as soon as surrounding stimulation is changed" (p. 106).

The restrictions which must be placed upon this statement of point of view are clearly indicated by Köhler. In physical processes there are two sets of factors which must be considered: the actual forces involved and the constant limiting or boundary conditions which restrain the former. Thus preëstablished anatomical conditions are present to some extent in vision and touch, but certainly the limiting conditions in learning and habit formation are not as hard and fast as they have been pictured, nor even as set as those in the sensory fields where *Gestalt* phenomena abound. Orderly functions may run their course without being controlled by special arrangements preëstablished *ad hoc*.

Nor is the alternative to definite paths and connections vitalism. Order may result through *dynamical interaction*. To the reviewer's mind the most important and far-reaching statement in the book is the following: "Dynamical interaction, undisturbed by accidential impacts from without, leads to orderly distribution, though there are no special regulative arrangements, and definite direction arises in such systems from purely dynamical physical principles" (pp. 138 f.). This statement is diametrically opposed to the commonly held view of the consequences of the law of entropy. That some such view must be true follows from the fact that we do find order and directedness in physical processes which are in direct conflict with the second law of thermodynamics. Köhler at this point is more than psychologist, and *Gestalttheorie* becomes more than a new brand of psychology. Here is an attempt to derive order and arrangement from dynamical considerations which leave behind the simple

mechanisms and atomism of 19th century physics without an appeal to *vis viva*, *élan vital*, entelechy or any other vitalistic principles. We have here a philosophy of nature reaching down into the very foundations of modern physics.

There is one change in *Gestalttheorie*, prominent in this book, which will help us to understand Köhler's position in general. Whereas in earlier writings the emphasis has been on how things get together to form *Gestalten* (*e.g.* Wertheimer's short circuit theory), Köhler continually stresses the point that *segregated wholes or units* form the basis of this latest exposition of configurationism. This is an important change, and to the reviewer it is the outstanding contribution of the book. The change relieves *Gestalttheorie* from embarrassing questions and makes the concept of *Gestalt* less vague and mysterious. It will be no longer necessary to write at length, as Köhler was forced to do in *Physische Gestalten*, on the problem of independent or isolated systems. The concept of *Gestalt* need no longer be an ambiguous thing referring to almost any connected series of events, logical, perceptual, behavioral. *Gestalten* are those segregated wholes met with in nature, the objects fashioned by man, and those processes both in nature and the organism which can be seen and understood only as unities, possessing specific properties as such. From this point of view forces work toward separation, delimitation, and concrete unities as well as in the interests of fusion and amalgamation. Form, which previously did and did not seem to be the *Gestalt*, now emerges as "in some respects the most important property which a whole may have" (p. 219.) As a clarification of this point alone (a consummation devoutly wished by friends of the school) the book has made a great step forward.

We come to Köhler's treatment of certain crucial topics: meaning, association, reproduction, behavior, and insight. The treatment of *meaning*, which is scattered throughout the book, engages Köhler almost from the very beginning, for we are told in the third chapter that meaning has been the bugbear of the introspective school. Köhler's theory of meaning, if he may be said to have one, seems to be that meaning is immanent, a property of organized wholes, following lines drawn by natural organization. Since segregation precedes meaning, the latter cannot be used to explain the former. Nor does the concept of 'past experience' help to explain why a given object has this meaning here and now, since present organization seems to be the determining factor. For the meanings given in cross-section, the kind dealt with mainly by Köhler, the theory seems adequate enough, but the reviewer doubts if the temporally extended events involving the growth and decay of meanings can be treated in this way. The application of configurational concepts to the temporal cases has been promised for a number of years but still remains to be worked out with the certainty and force that characterize the static cases.

Association and *reproduction* are presented as amenable to the same principles Köhler has enunciated for perceptual patterns. The form of the segregated whole determines what will be associated or reproduced from past experience. It is this which distinguishes *Gestalttheorie* from such a theory as redintegration which does not provide for the differentia of association and reproduction. "If association as a basis for reproduction is just another expression of the fact that the traces of functional wholes are themselves

detached wholes, one might somewhat rashly deduce from this theory that, after a trace is formed, any group of stimuli which represents a considerable fraction of the original constellation of stimuli will reproduce those parts of the original process the stimuli for which are not actually given" (p. 311). Association and reproduction emerge as subordinate to the principles of organization.

The chapter on *behavior* will come as a surprise to many who will find the method of approach to behavior much different from that common in this country. The problem which Köhler attacks is that which concerns the manner in which we come to ascribe to others experiences similar to our own. Indeed, others may judge my feelings by my behavior better than I myself can. The manner of speech may be more indicative of attitude or of meaning than what is actually said. Even inanimate objects and events may, on the face of them, bear aspects which can only be described in such anthropomorphic terms as 'threatening,' 'fearsome' or 'alarming.' There is no dualism between what we see on the behavior side and what is directly experienced from within. There are glimmerings here of a new theory of empathy and the whole discussion points toward a *verstehende Psychologie* which as yet is almost purely descriptive.

We have now arrived at the last and most difficult topic in the book—*insight*. The importance of this concept is indicated in the statement that "the chief dividing line among contemporary psychologists would separate those who acknowledge obvious, direct determination as it appears in common experience from those who can only admit indirect connections" (p. 383). By *insight* is meant the fact that we may know that certain things are related as we experience them, without having to learn or go through a process of conditioning to arrive at the thought. We see that our attitudes arise from certain situations and that, in turn, situations may be colored by our already formed attitudes. Köhler admits that there are many cases of behavior in which insight is lacking. And when insight is admittedly a product of learning, a problem still remains as to what degree of dynamical relationship exists between the parts of a situation. The theory is finally stated in terms of central brain processes which may help to make it more concrete for some psychologists.

To evaluate a book of this kind, written partly for the layman, is extremely difficult. That it is not Köhler's *magnum opus* the reviewer feels very sure, preferring to reserve the palm for *Physische Gestalten*.

In one or two places, the reviewer would ask for more than has been given. Thus he feels that, in spite of the objections which may be urged against behaviorism, it has been a most productive movement in this country, furthering the study of animal and child behavior, the establishment of institutes and clinics, and providing an incentive for a more functional view of the organism. Of these facts Köhler does not seem to be aware. Again, where we might have expected some word on such a moot problem as the configurationist's position with regard to analysis, nothing is said. In answering critics, Köhler's failure to mention the names of the persons he is discussing leaves a question in the reader's mind as to what the critic really said or meant. The lack of specific references to names and the literature lends an air of thinness to many

of the arguments and counter-arguments which might easily have been avoided. However, if one trusts his author and enjoys an outing on a "Sunday of research" this book will come as a pleasant, perhaps shocking experience which, in any case, is worth having.

Bryn Mawr College HARRY HELSON

Lehre von den Gesichtsempfindungen auf Grund hinterlassener Aufzeichnungen. By FRANZ HILLEBRAND. Edited by Franziska Hillebrand. Vienna, Julius Springer, 1929. Pp. v, 205.

Hillebrand's work was devoted largely to investigations of visual phenomena. His chief contributions cover the period of 1889-1922. It had for some time been his plan to bring together and organize the results of his researches in a single critical volume, but this work was cut short by his death. The present volume is edited with annotations by his wife, Dr. Franziska Hillebrand. Since its contents have largely appeared in earlier publications it is unnecessary to review it critically here. The volume is a detailed and critical exposition of the general field of visual sensations and perception rather than a new contribution to a particular problem. The first part analyzes color phenomena and investigates thoroughly their relationships to variations and complexities of external stimuli and to retinal adaptations and contrast effects. The extreme complexities of these relationships are well brought out and illustrated by numerous figures, and it is shown that hue, saturation, and brightness do not have a one to one relation to the physical variables in the stimulus. Hillebrand recognizes the defects of both the cylinder and the prism as means of representing the interrelationships of the factors in color phenomena. He maintains that the W-Bk series is really not a series of brightnesses but one that *has* brightness, since black is not the mere absence of something. The energy curve of the light-intensity series is an entirely different thing from the brightness curve as determined photometrically; and hue, brightness, and saturation are not variables which are independent of one another. In this discussion the activity of the retina receives much emphasis. There is a technical discussion of the laws and principles of color mixing, in which the non-predictability of the results of different mixtures is emphasized as well as the necessity of depending always on empirical procedures. A critical evaluation of color theories completes Part I. One is disappointed to find that the Ladd-Franklin theory is not even mentioned either by the author or the editor.

Part II is devoted to visual space. It is here that the author has done his most original work. Hillebrand is a nativist, as is well known; so for him this topic is as much a matter of sensation as is that treated in Part I. The geometrical aspects of localization are stressed at the expense of the genetic aspects, and localization *in situ* is thoroughly considered. Comparisons between 'sensed' and 'real' space are frequent. For single vision "the stimulation of any retinal point produces in consciousness a single, completely determined direction of vision" (p. 104). In the case of binocular vision, therefore, "The sole source of the perception of depth is single vision with disparate retinal points" (p. 125). Without such single vision we could see the double images resulting from stimulation of non-corresponding points only as crossed or uncrossed,

not as far or near. Muscular strains of accommodation and of convergence are of secondary and minor importance. On this point Hillebrand came into conflict with both Wundt and Helmholtz. Hillebrand, because of this position and his acceptance of the nativistic view of visual space perception. devotes considerable space to a detailed consideration of the horopter. In his critique of empiricism he dismisses the effects of the 'feelings' of muscular movement and of impulses as factors in visual depth, with the assertion that a view based on such factors only assumes what was to be explained; if no spatial factor is natively present, none can be built up by the experience of these muscular impulses. Therefore, he contends, any assumption of spatial perception from muscular activity is only another form of nativism. In similar fashion he meets objections to his view brought in from the developmental factors in children and from the experiences of persons blind from birth with cataract and made to see by operation. Hillebrand saw only the alternatives, mentioned above, of conscious retinal factors or conscious muscular factors; he did not recognize the localization effects of muscular contractions of which one is not directly aware, in one's reactions to space relations or situations. (See the reviewer's article in *Psychol. Rev.*, 1926, 33, 218-236.) We cannot here enter into the controversies of nativism versus empiricism, but it must be noted that the so-called secondary factors are rather efficient in the case of vision with one eye only; moreover, directly in opposition to Hillebrand's main source of depth perception as quoted above—single vision by corresponding points—one may recall that wall-eyed and cross-eyed individuals must acquire new corresponding points.

This monograph, summarizing the experiments on visual phenomena from the standpoint of Hillebrand's work, is well worth owning by experimental psychologists. J. P.

The Psychology of the Adolescent. By LETA S. HOLLINGWORTH. New York, D. Appleton and Co., 1928. Pp. xii, 227.

The Psychology of Adolescence. By FOWLER D. BROOKS. Boston, Houghton Mifflin Co., 1929. Pp. xxiii, 652.

The titles of these two books connote in some degree the general difference in the expositions. Both books are concerned with much besides the psychology of youth, unless that term be taken in one of its more popular uses. Both authors use the results of educational research and of psycho-clinical, biological, and sociological studies of the pre-adolescent and adolescent years. Dr. Hollingworth's study of the problems of the average adolescent has a strong clinical and anthropological tone throughout. Dr. Brooks, in presenting the facts of hundreds of individual investigations into the physical, mental and social factors in the period of adolescence has necessarily presented much statistical material as well as many psycho-clinical details of mental hygiene. Hollingworth addresses her book to parents and teachers, particularly to those who have the care of children in the pre-adolescent years, as she wisely believes that the maximal gain results when adolescence is anticipated in the rearing of the infant and the child. Brooks sets himself the task of "describing adolescent nature, growth, and development so as to facilitate both reliable prediction

and suitable guidance and control of behavior during the teens." The first author states that much of the "lore about adolescence" rests "upon the mere opinions of professional observers, rather than upon exact quantitative researches." The second presents in much detail the impartial evidence of such researches, adding his own critical analyses and conclusions. Both books contain an index and selected references. Brooks has placed a list of references at the end of each chapter as well as a list of problems for discussion. In addition he has appended a glossary of technical terms. Both books are valuable and important additions to the current literature of youth. Hollingworth's volume is for the intelligent and inquiring layman, while *The Psychology of Adolescence* may be considered more in the nature of a text and reference book for those who desire to study in greater detail the whole problem of this period. The general point of view of both authors is that the continuity of life is not broken either at the onset of puberty or upon the attainment of adulthood.

To illustrate the difference in exposition and the point of view in the two books let us take a specific statement from each author. Hollingworth holds that "the attainment of puberty is positively correlated with intelligence," that the gifted child will attain puberty earlier than the average and much earlier than the feebleminded. In his Chapter VI, Brooks examines critically the correlation and significance of physical and mental growth at and during adolescence, discussing various kinds of maturity. After citing impartially a great many investigations, he concludes that the correlation between physical maturity and mental status is so low that "predicting mental or scholastic ability from physical traits is only a little more accurate than guesswork." As extreme instances he mentions two cases of puberty praecox of Gesell's, one a girl who matured at the chronological age of three and one-half years and developed physically with great rapidity but mentally as an average normal child, the other a girl who matured at eight years but remained an imbecile although her height and weight were normal or above. Here is expressed not merely a difference of opinion but a fundamental difference in method as between Brooks's exhaustive examination of the facts available and Hollingworth's acceptance of general, clinical opinion based upon observation and, in this case, on Terman's *Genetic Studies of Genius*. There is no implication that one book is inaccurate or superficial while the other is exact and careful; but in this instance the weight of evidence seems in favor of Brooks. Whenever possible throughout his book, Brooks so presents his material that the reader may judge for himself, while Hollingworth, addressing the parent, is descriptive, explanatory and generalized.

These books generally complement each other. Each deserves attentive reading. Brooks's genetic and pedagogical psychology, based on original nature, learning and interests, is chiefly that of Teacher's College at Columbia. His volume includes a chapter on the "Disturbances of Adolescent Personality" and one on "Prediction of Adolescent Behavior" where the correlational methods are succinctly and admirably explained. Hollingworth is concerned with the average normal adolescent and does not discuss either psychoses or neuroses. Her chapter on "Psychological Weaning" is a masterly exposition of how parents can help or hinder a child to pass from dependency to a free and adequate maturity, and her discussions of the mental hygiene of the in-

dustrial life of the young and of the social and psychological factors involved in mating are valuable contributions to the study of adolescence. Her volume was chosen by the American Library Association for the League of Nation's Committee on Intellectual Coöperation.

Cornell University MARGARET RUSSELL BENTLEY

The Mind of the Savage. By RAOUL ALLIER. New York, Harcourt, Brace & Co., no date. Pp. xiv, 301.

This volume is a translation of *Le non-civilisé et nous* (1927), a sequel to the more ponderous *Psychologie de la conversion chez les peuples non-civilisés* which appeared in 1925. In spite of the obvious bias of its author, the book is eminently readable. To the student of the 'collective mind,' the 'social' psychologist, it offers a wealth of illustrative material, drawn from a variety of sources. The initial chapter-heads (Magic and the Arrest of the Intelligence, and Magic and Moral Disintegration) afford a clue to the general drift of the argument. The task set himself by the author (who is dean of the Protestant Faculty of Theology at Paris, with a major interest in the colonial missions and native tribes of Madagascar) is the charting of the gulf which separates the civilized from the surviving primitive. Is the difference in mentality posited by Lévy-Bruhl irreducible, he queries, the mark of a divergent evolutionary trend? Or are the uncivilized simply retrogressive communities of the main stock?

Following Gräbner's lead, Allier summarily dismisses evolutionary argument, deciding in favor of cultural diffusion. Arrested development or deterioration is the sequel, not of evolutionary factors, but of the wilful adoption of magical belief and practice. Reliance on an omnipresent *mana*, controllable through charms (*vuriké*) and the offices of the witch-doctor, paralyzes observation and reason, and demoralizes character. Life among the Bantus and other African tribes is luridly depicted as a long nightmare of terror and illusion, a kind of chronic paranoia, with suspicion directed continually against one's associates and family. The activities of secret societies of sorcerers (*Wihibis*), whose rites demand human sacrifice, foment the psychosis.

The remedy is moral and emotional, rather than intellectual, regeneration (in short, conversion), if the findings of Durkheim and Lévy-Bruhl as to the mystic, pre-logical character of mental processes in primitives are to be trusted. The feasibility of reclaiming these backward members of the race is guaranteed by certain elements of identity in human nature, discoverable everywhere under apparent difference. Two of the most illuminating sections of the book are devoted to establishing this thesis (Chs. IV and V). Evidence bearing on the recrudescence of magic and the use of charms and amulets during the recent war, especially among the soldiers draughted from civilized nations, is given in detail. These and other modern instances of semi-magical beliefs and reactions among the civilized are classified by Allier not as survivals, working up from lower strata of culture, but rather as *reinventions* of magic. Binet, Piaget, Lévy-Bruhl and others are cited in support of a universal anthropocentric trend. The bent to construe emotionally rather than cognitively—in short, to *participate*, in all perceptual processes—is present, especially in childhood, no matter what the cultural level.

The sources utilized in the account of *vuriké* and voodoo are in the main identical with those employed by Lévy-Bruhl—the memoirs, in large part, of African missionaries and governors, Moffat, Junod, Julien. Allier's marshalling of evidence in support of his own hypotheses, however, rarely convinces. *E.g.*, the data, *as presented*, would seem, to the reviewer's mind, to point to 'germinal arrest' rather than magical practices as the causal factor in savage backwardness. Evangelical fervor, one suspects, may color one's perceptions no less insidiously than do magical prepossessions; and the phenomenon of participation may be commoner even than our author would be willing to admit. Cf. especially the construction put upon native suicides (80 ff.), upon Arabian voluptuousness (247 ff.), and upon the effect of the hunt upon the character of the hunter (245 ff.).

The book is provided with a usable index (though of proper names only); and there are appendices on the belief in men-tigers, on the system of vendetta among savages, and on the so-called 'instinct of orientation.' The translation of the text is hardly of uniform excellence, sometimes, in fact, absolutely misleading. Yet with all its defects the work is an interesting document, an attempt to link up modern theories and the findings of the social sciences with the practical problems of colonization.

Cornell University ELSIE MURRAY

Le développement mental et l'intelligence. By HENRI PIÉRON. Paris, Félix Alcan, 1929. Pp. xii, 97.

This small volume consists of four lectures which the author gave in the University of Barcelona. The intention in these lectures was merely to trace the main lines of progress and to indicate the chief problems that had arisen in this field of psychology. The first lecture takes up the stages of mental development. The author emphasizes the general organic unity so significantly brought out in modern biology, and he recognizes the purely practical motives in separating physical and mental growth, as well as the abstract and isolated picture set up by science as compared with the living individual. The great variability in the mere appearance of this or that definite activity or aspect of physical development, he maintains, must be studied in a close and correlated manner. He points out that even at birth a certain level of development (at least the neural) might be worked out and any given new-born child classed as retarded or advanced, but nervous development can be correctly based only on a variety of behavior manifestations instead of on single acts. He is certainly on somewhat questionable ground when he maintains that early walking acts depend solely on certain neural maturations. Piéron admits that physical and mental retardations are not closely related, being somewhat differentially affected by hormones, etc. He is somewhat too positive as to the underlying organic causes of certain variations from normal development; we can not go ahead of our verified hypotheses. Mental development is characterized as the general capacity of coördination, of the unification and stabilization of conduct. It is adaptation not only to present but also to future and remote circumstances through mechanisms not made clear. Future circumstances are, of course, aspects of present conditions. The use of symbols, particularly in language, is

important in higher mental development and is stimulated by 'social instincts,' including impulses to imitate.

The second lecture traces the development of intelligence scales from Binet down through the American and European countries, and tests for persons laboring under certain handicaps like deafness are also included. The "fundamental postulate" in the application of any scale, he maintains, is that all subjects have been subjected to comparable educational and social influences. The next lecture takes up levels and profiles of mental development, presenting different theories of mental growth in individuals and races. One certainly needs more evidence than Piéron gives (or than exists yet) for holding that abilities in some races, e.g. negroid, manifest natively higher levels at first and later show earlier attainment of limits than do other races. This is a pretty theory but is not supported in test data thus far obtained. Profile schemes like those of Rossolimo and Lasoursky of Russia, Vermeylen of Belgium, are briefly presented and evaluated. The work of Downey and of Seashore is not cited. It is recognized that the reliability of such various traits is generally not well established. The last lecture takes up the problems of analysis of intellectual capacity and reviews the theories of Spearman and others, now so well known. To locate centers of automatic activity, instinct, "the purely affective life" in mammals in the striate bodies and thalamus he cites the work of Rothmann, Zeilony and Dusser but makes no reference to Cannon's and Stone's work on these subjects nor to the study of more general mental functions by Lashley, though his view does not differ significantly from that to which Lashley's work has led regarding integrated mechanisms. Piéron points out that a rather large percentage of the 'genius' manifested in certain of our American tests for children (based upon Terman's classification) does not seem to be present in adult society. He wonders whether the sort of profundity seen in the work of Newton, Einstein, etc., directed to the solution of problems irrespective of the time required, should not be given more consideration in testing. The mere rudimentary nature of various analytic techniques in present-day testing is indicated.

The booklet is a more detailed, critical, and comprehensive piece of work than most similar American surveys or summaries, and is well worth reading.

J. P.

The Influence of Habit on the Faculty of Thinking. By MAINE DE BIRAN. Tr. by Margaret D. Boehm. Introduction by George Boas. Baltimore, The Williams and Wilkins Co., 1929. Pp. 227.

This is the third of the series of *Psychological Classics* edited by Knight Dunlap. It is valuable to English readers in making more easily available to them the classical work of a writer diverging somewhat from the views of the earlier French school called "ideologists" and laying the basis in important respects of the French 'spiritualism' which culminated in the works of Henri Bergson. The translated volume contains, besides the illuminating historical Introduction by Boas, a report of a committee appointed by the Academy of Moral and Political Sciences of the Institute of France, to examine and evaluate seven memoirs entered in competition upon the question "To determine what is

the influence of habit upon the faculty of thinking; or, in other words, to show the effect which the frequent repetition of the same performances produces upon each of our intellectual faculties." The first contest brought no results, as none of the contributions was regarded as fulfilling the designs of the Institute. The second competition resulted in the award of the prize to the author (Maine de Biran) of the present work; and M. Destutt-Tracy of the Committee, who was selected to write the report, makes an exposition and analysis of the contribution, covering 17 pages. It should be said that Maine de Biran's paper had been offered before and had received honorable mention as well as criticism and advice from which the author had profited in the re-writing.

The subject is clearly a psychological one but was attacked in that day (1803), as such problems were then, by a non-experimental method. Psychologists interested in the historical background of their subject will find themselves much indebted to the persons who have made available this excellent translation of an important monograph which marks the point of divergence of French 'will' psychology from the earlier extreme developments of associationism. This indebtedness is real whatever they may think of some of the modern fruits of this divergence and of the influence of Maine de Biran on the development of experimental psychology in France.

J. P.

Problems of Instinct and Intelligence. By R. W. G. HINGSTON. New York, The Macmillan Company, 1929. Pp. xiii, 296.

Major Hingston is a 'naturalist' who writes in fluent style a book which is best reviewed by reference to the extremely cautious preface written for it by Bertrand Russell. After some general and complimentary remarks, Russell is impelled to point out that Hingston's treatment of insect memory "goes beyond what his data warrant" and that his view of mental evolution "assumes the inheritance of acquired characters in a very extreme form" (p. xii). The very presence of such criticisms in the introduction may serve as a warning that the reader must proceed at his own risk in accepting without question Hingston's not infrequently startling conclusions.

The author obtains most of his factual material from his own observations in the field, supplementing this, however, with a well-documented supply of quotations, chiefly from the Peckhams, Forel and Fabre. His early chapters advance evidence of this sort regarding the perfection, wisdom, rhythm and variability of instinct, as well as its limitations, flexibility and folly. He is convinced that insect behavior is not limited to instinctive patterns, but that it includes, as well, behavior typical of memory, of intelligence, and of an 'unknown sense' which serves to explain the activity of migration. Observing largely among the Hymenopters, he finds many cases in which insects depart from conventional patterns of behavior in meeting novel situations. Thus he describes how ants build bridges and platforms to help one another, how wasps vary their methods of attack under special circumstances, and how other insects meet new situations 'intelligently.'

Hingston holds that "We cannot explain psychic phenomena by reducing things to physical and chemical laws. Even insect psychology has something else. It has memory and conscious mind" (p. 247). Instinct, he finds, "began

in a reasoned act . . . this act, through being continually repeated, tended to lose the reasoning element and to become more and more unconscious. As this process continued through generations, the mental machinery by which it worked got more indelibly engraven in the mind. And in the end it became automatic—in other words, it became instinctive" (p. 268).

There is a wealth of descriptive material in the book, but it is to be regretted that the author chose to illustrate entirely by sketches, without the use of photographs. There is a singular lack of negative evidence, as well as an irritating refusal to define terms, both of which detract from the value of the treatment. Thus Hingston refuses to define his terms 'instinct' and 'intelligence,' because he regards them as "incapable of accurate definition," and "everyone knows what is meant" by these terms. His writing is anthropomorphic throughout the volume; thus he finds that an ant returns to a former feeding place because "the thought of food has recalled to its memory its experiences of twelve hours before" (p. 242), while a wasp acts preoccupied because "her attention is absorbed in energetic effort; hence she cannot fix it on other things" (p. 237).

Iowa State College JOHN G. JENKINS

Educational Psychology. By RUDOLPH PINTNER. New York, Henry Holt & Co., 1929. Pp. xiv, 378.

The content of textbooks in educational psychology is ever changing, and very properly so. In this book the author has departed from the custom of devoting much space to neuro-anatomy, and has proceeded directly to the material that the practical educator needs and in which the up-to-date teacher of educational psychology is interested: The treatment of original nature, character education, and non-intellectual traits is of such a nature that it might well be called 'educational psychiatry." This is as it should be, for educational psychology should certainly apply in the matter of developing personality and character as well as in the matter of teaching facts.

There may be some objection to the use of the term "trait" as applied to character. It is true that people vary a great deal from time to time in honesty, industriousness, frankness, and other types of conduct, while "trait" indicates greater fixedness. There seems to be no other term, however, that can be substituted for this; no term that covers the meaning more satisfactorily. On page 71 the author says "When we study the conduct of an individual we find a set of habits of action, more or less loosely organized. These modes of reaction are based upon his original tendencies, but they have been conditioned by his environment." Growing out of these statements would seem to be the idea that a "trait" is a type of conduct, either inherited or acquired or both. For general practical purposes this concept should be satisfactory.

Dr. Pintner's 'divisions of intelligence' seems open to question. On page 128 he says: "The use of different instruments of intelligence measurement has brought to our attention the fact that there are different kinds of intelligence, such as abstract, concrete and social." Such a classification will hardly bear analysis, for it is in social life that abstract ideas function to the greatest extent. Also the concrete and the mechanical have their abstract and social aspects.

Furthermore, it may be noted that language falls into the classification of the abstract and that many individuals learn one language with much greater ease than they do another. Likewise, an individual may be very successful in dealing with one type of concrete situations or material but an absolute failure, in terms of modern efficiency, in dealing with another. It might be more nearly correct to speak of mechanical and social "insight" or "ability" if we agree that "insight" and "ability" are due to native capacity plus experience or training. In like manner we might speak of "political insight," "legal insight," or "financial insight," but we can hardly speak of political, legal, or financial intelligence. A politician, lawyer, or financier might be very successful in one situation, but have very little success in another in which he has had little or no experience.

The author's discussion of the practical application of intelligence testing in the matter of vocational guidance is unquestionably sound. On page 143 he says "In a broad sense all intelligence testing may be used for general educational and vocational guidance. In the narrower sense of giving specific educational advice as to whether to go to high school or college, or as to the course of study to be chosen, intelligence tests may help but they cannot be used in a mechanical or arbitrary way. The wise counselor will always make use of them but never depend upon them alone." From page 144, "Intelligence, however, prescribes the limits within which the individual must function."

The chapter on the "Laws of Learning" is too brief. The treatment is of a rather hurried type, such as an author sometimes gives to a matter that he thinks is already well known to every one. More time should have been given to discussion of the psychological principles back of these laws, to illustrations of their practical application, and to explanation of the co-functioning of these laws in the case of the most effective learning. It would seem, also, that some mention should have been made of the synaptic and the *Gestalt* theories of how learning takes place. The other chapters on learning, however, are much fuller, including the matter of efficient learning, practice curves, improvement curves, motivation, and the curve of forgetting.

The discussions of "New-Type Examinations" and the use of measurements in educational work are reasonably adequate and very much to the point. It seems obvious that if education is ever to become a science we must recognize the fact that the measurement of the results of our teaching is just as important as the use of the best psychological methods in daily classroom instruction.

Teachers College, Syracuse University ROBERT P. CARROLL

AN ACKNOWLEDGMENT

The portrait of Hermann Ebbinghaus, which appears in this number of the Journal, was reprinted from the frontispiece of the *Zeitschrift für Psychologie*, vol. 51, 1909. The signature which appears below it is a facsimile of one in the collection of autographs owned by the Department of Psychology of Cornell University. K. M. D.

INDEX

By J. P. GUILFORD, University of Nebraska

AUTHORS

(The names of authors of original articles are printed in CAPITALS; of authors of books reviewed, in roman; and of reviewers, in *italics*.)

Abderhalden, E. 171
ABEL, T. M. 321
Abel, T. M. *168*
Addington, B. H. 171
Allier, R. 665
ALLISON, L. W. 634
Allison, L. W. *492*
ANDERSON, O. D. 345
Anon. 493
Augier, E. 343
Aveling, F. A. P. 470

Baar, J. 489
Bagley, W. C. 484
Baylor, E. M. H. 169
Beebe-Center, J. G. *503*
BENTLEY, M. 115, 116, 147, 320, 429, 519
Bentley, M. *148, 504*
Bentley, M. R. *663*
Bertrand-Barraud, D. 503
Betz, W. 165
Biran, M. de 667
BISHOP, H. G. 38
Blanchard, P. 477
Bode, B. H. 490
BODER, D. P. 107
BORING, E. G. 308, 319, 444, 446, 469
Boring, E. G. 654
Boring, E. G. *156*
Boutroux, E. 501
BOYNTON, M. A. 270
Bréhier, E. 166
Bronner, A. F. 169
Brooks, F. D. 663
Brown, A. W. 490
Brownell, W. A. *477, 492, 494*
Bruni, G. 503
BRUSH, E. N. 408
Buyse, H. J. J. 488

Cannon, W. B. 322
Carmichael, L. *342, 490, 500*
Carr, H. W. 162, 500
Carroll, R. P. *669*
Challaye, F. 167
Chevalier, J. 336
CHOU, S. K. 303
Cobb, S. 502
Cocks, A. W. 491
Collingwood, R. G. 470
Colvin, S. S. 484
Conklin, E. S. *169*
COOK, B. 246
COREY, S. M. 439
CRAFTS, L. W. 591
Crosland, H. R. *503*
CUFF, N. B. 639
CURETON, E. E. 235, 405, 608

DALLENBACH, K. M. 72, 116, 306, 321, 423, 458, 469, 637, 670
Dallenbach, K. M. *153*
Dashiell, J. F. 479
DAVIS, R. C. 300
Descoeudres, A. 504
Dimmick, F. L. *161*
DODGE, R. 295
Drake, D. 503
DUNLAP, J. W. 235, 405, 608
Dwelshauvers, G. 486

Easley, H. *480*
EBERSBACH, R. 413
Edgerton, H. A. 482
Eldridge, S. 497
ELLIOT, M. H. 315
ESTABROOKS, G. H. 115
Ewen, J. H. 496

FAUCETT, M. A. 631
FERNBERGER, S. W. 317, 646
FERRALL, S. C. 72
FERREE, C. E. 63
Fletcher, H. 326
FORBES, T. W. 114
FREEMAN, E. 287
Freeman, E. *336*
Freeman, F. S. *502, 504*
FREEMAN, G. L. 173, 581, 636

Garrison, S. C. *171, 503*
Gee, W. 497
GENGERELLI, J. A. 399
Giese, F. 494
GLAZE, J. A. 628
GOODENOUGH, F. L. 270
Goodenough, F. L. *502*
GRAHAM, C. H. 420
GUILFORD, J. P. 415, 436, 671
GUNDLACH, R. 519

Haak, K. 161
Hadfield, J. A. 470
Healy, W. 169
Helson, H. *657*
Hillebrand, F. 662
Hingston, R. W. G. 668
Hobhouse, L. T. 470
HOLLINGWORTH, H. L. 457
Hollingworth, L. S. 663
Hilsopple, J. Q. *344*
Holzinger, K. J. 502
Höningswald, R. 164
Howells, T. H. *482*
HUGHES, E. 412
HULL, C. L. 279, 442
Hull, C. L. 334
HUMES, J. F. 1
Hunter, W. S. 495
Husband, R. W. *485*
HUSE, B. 279
HUTT, M. L. 450
Huxley, J. S. 470

JACKSON, H. 96
JACOBS, E. 414
Jacobson, E. 473
Jenkins, J. G. *668*

Kabanov, N. A. 504
Kantor, J. R. *495*
Kelley, T. L. 338, 504
Kellogg, W. N. 105, 300
Kenneth, J. H. 389
Klüver, H. 156
Koch, H. L. 370
Koehler, W. 657
Koos, L. V. 499
Kraehenbuehl, J.O. 631
Krueger, R. G. 302, 442

Laing, H. E. 490
Landis, C. 431
Lanier, L. H. *172, 473*
Lauer, A. R. 298
Lehmann, H. C. 140
Lindemann, F. A. 470
Lindley, S. B. 301
Lossky, N. 499
Lowenstein, E. 423
Lund, F. H. 51

Mackenzie, M. 414
Matthews, W. R. 470
McDowall, R. J. S. 470
McGeogh, J. A. 455
Meenes, M. 260
Metfessel, M. 638
Miller, D. 246
Mitchell, B. C. 502
Monroe, M. M. 63
Moore, M. G. 453
Moul, E. R. 544
Moul, E. R. *172*
Murchison, C. 153
Murphy, G. 156
Murphy, J. P. 169
Murray, E. 117, 640
Murray, E. *665*

Nöh, E. J. 415
Noltenius, F. 171

Oberlin, K. W. 621
Ogden, R. M. *326*
Orata, P. T. 487

Papez, J. W. 342
Paterson, D. G. 101
Patrick, G. T. W. 485
Pattie, F. A. 319
Paynter, R. H. 477
Pear, T. H. 500
Peterson, J. 459, 561
Peterson, J. *167, 322, 327, 334, 338, 475, 484, 489, 490, 494, 496, 498, 499, 500, 503, 662, 666, 667*
Peucesco, M. G. 170
Piéron, H. 666
Piersel, W. G. *503*
Pintner, R. 669
Plaut, P. 331

Rabaud, É. 168
Rand, G. 63
Reiner, H. 172
Reynier, M. 167
Ring, C. C. 429
Roberts, D. 96
Ruch, G. M. 504
Ruckmick, C. A. 106, 638, 650
Ruckmick, C. A. *164, 165, 170, 470, 486, 496, 654*

Sadler, W. S. 480
Sanborn, H. *166, 331, 488, 499, 501*
Scheidemann, N. V. 172
Schulte, R. W. 496
Scofield, C. F. 213
Searles, H. L. 169
Seligmann, C. G. 470
Shakow, D. 505
Sherman, I. C. 492
Sherman, M. 492
Sligh, G. 412
Smith, F. W. 561
Spearman, C. 645
Squires, P. C. 134, 445
Stekel, W. 504
Stewart, C. 412
Stout, G. F. 172
Sutherland, A. H. *483, 497*

Tapper, B. *162*
Taylor, W. S. 344
Terman, L. M. 504
Thouless, R. H. 389
Tiffin, J. 638
Tinker, M. A. 96, 101
Titchener, E. B. 148
Toops, H. A. 482
Travis, R. C. 295
Troland, L. T. 327, 483

Uhrbrock, R. S. 501

Valentine, P. F. 503
Van Briessen, M. 475
Vernon, P. E. 127

Wagoner, L. C. *501*
Walker, H. M. 498
Walsh, W. S. 502
Walton, A. 109
Washburn, M. F. 412, 413, 414
Weber, C. O. 647
Weiss, A. P. *171, 172, 343, 500*
Wells, E. F. 573
Wells, E. F. *479*
Wemple, L. 246
White, R. K. 431
Wilke, M. 436
Williams, G. W. 83, 442
Wilson, J. D. 470
Winsor, A. L. 602
Witty, P. A. 140
Woodrow, H. *487, 504*

Young, P. T. 17

Zigler, M. J. 246

SUBJECTS

(References in *italic figures* are to the reviews.)

Ability, general and specific, 608-620; theories of, *334 f.*, *338-340*
Abnormal psychology, *480-482*; Morton Prince and, *344*
Accommodation, and visual acuity, 293
Acuity, visual, factors in, 287-294
Adolescence, psychology of, *663-665*
Aesthetic, concept of, 640-644; value of profiles, 431-435
Affection, immediacy of, 621-627; method in, 35-37; theory, 27-35
Affective, attitude, 354 f.; consciousness, *503*; psychology, *171*; reaction to tones, 11 f.; sensitiveness, 413; values, constancy of, 17-27; value of odors, 17-27
After-image, temperature, 82; tones, 10, 38-50
Age, and visual acuity, 68 f.
Animal psychology, *see* Comparative psychology
Apparatus, auditory, 39-41, 526-528; automatograph, 105 f.; bradyscope, 303-307; catalepsy, registration, 83-88; colored papers, 445; conditioning, demonstration, 110 f.; continuous graphic record, 636 f.; counting, 631-633; ergograph, self recording, 111-114; eyelid response, 634 f.; facial model, 436-439; galvanic electrodes, 106 f., 298-300; light bulbs, 300 f.; manoptometer, 639; maze, demonstrational, 109; maze, elevated skeleton, 439-442; maze, tridimensional, 107 f.; memory, 371; optical fixation, 116, 218; oscillator, 347 f.; oscillometer, 86-88; perimeter buttons, 116; phonographic suggestions, 115 f., 442-444; phonophotography, 638 f.; planimeter, 86 f.; records of 'filtered' music, 115; rotation table, 637 f.; screen, observation, 114 f.; sound, localization, 638; tachistoscope, 303-307; tambour stylus, 302 f.; testing machine, 468; timing, 631-633; time recorder, 301 f.; tonus, measurement, 581-590
Applied psychology, *171*
Apprehension, visual span, 96-100
Aptitude, testing, *334-336*
Art, psychology of, *331-333*, 640-644
Association, context and, 172-212; Gestalt theory, *660 f.*; sex differences, 415-419
Athletics, psychology of, *496 f.*
Attention, in discrimination, 12; measurement of, *460 f.*
Attenuation, correction for, 235-245; 405-407
Attitude, affective, 354 f.; and affective judgment, 22-25; constancy of, 317-318; critical, 352 f.; and feeling, 573-580; observational, 345-369
Attributes, color, 621 ff.; tones, 524-543
Aubert-Förster phenomenon, 289
Auditory, localization, 519-524

Behaviorism, *658*, *661*
Bergson, philosopher, *336-338*
Blind-spot, 651; movement in, *222-223*; vision in, 213-234
Brightness, discrimination, 651; of tones, 528-542

Catalepsy, hypnotic *vs.* voluntary, 83-95
Character, education, *493 f.*; judgment from profiles, 431-435
Chemistry, of blood and performance, 652
Child, psychology, *157*, *171*, *666 f.*; problems, *477 f.*; sleep of, 270-278
Clinical psychology, *477 f.*
Color, attributes, 621 ff.; blindness, 117-125; film, 544-560; phenomena in flicker, *161 f.*; problems in, 117-127; retinal rivalry of, 262-266; thresholds, 651; theories of, 119-125
Comparative psychology, *165 f.*, *168*; orientation in, 413 f.
Conditioned reflex, *330*; class demonstration, 110 f.
Configuration, *see* Gestalt; in flicker, *161 f.*
Consonance, and dissonance, 45-49; modifiability of, 561-572; musical training and, 563-572; practice and, 566-571
Context, in associative formation, 173-212
Contiguity, in association, 174
Cretin, history of concept, 319 f.
Cutaneou ssensitivity, in regeneration, *461 f.*

Dehydration, and parotid secretion, 602-607
Depth, in retinal rivalry, 262-266
Descartes, philosophy of, *601*
Dextrality, and eyedness, *460*, 653; and orientation, 54-62; in rats, 651
Differential psychology, 646 f.
Discrimination, tonal, 1-16

Ebbinghaus, bibliography of, 516-518; biography of, 505-516, portrait of, 505, 670
Education, *502*; of mental defectives, *504*
Educational psychology, *669 f.*
Eidetic images, *see* Images
Emotion, expression, facial model, 436-439; expression, posture, 653; introspective study, 573-580; physiology of, *322-325*; and relaxation, *474*; theory of, *324 f.*, *475*
Epicritic sensitivity, 447
Epilepsy, dehydration and, 602-607
Epistemology, *499*
Ethics, *167 f.*; and religion, *162-164*
Evolution, and ethics, *162-164*
Examination, true-false, *419 f.*
Experimental psychology, *172*; history, *654-657*
Extraversion-introversion, and flicker phenomenon, 412; tests, 412 f., *653*
Eyedness, *see* Dextrality
Eyelid responses, 634 f.

Facilitation, social, 320
Fatigue, and practice, 628-630
Feeble-minded, education of, *504*
Feeling, attitude and, 573-580
Figure-ground, 444 f.; in peripheral vision, 251-256; in retinal rivalry, 266-268
Flicker, and extraversion-introversion, 412; phenomena, *161 f.*
Form, perception, 63-71, 246-259
Frequency, law of, 453-455

Genetic psychology, primitive mentality, *665 f.*
Gestalt, *vs.* experience, 453-455; in maze learning, 652; psychology, 308-315, *337 f.*, *657-662*; in rhythm, *164 f.*; and structuralism, 134-140; theory of transfer, *487 f.*

Habit and thought, *667 f.*
Habituation, to suggestion, 284
Handedness, *see* Dextrality
Hearing and speech, *326 f.*
Heat, analysis and synthesis of, 72-82; burning, 423-429; critical temperatures for, 423-429; quality of, 76 f.
Hedonism, *331*
History, experimental psychology, *654-657*; psychology, *153-156*, *156-161*, 505-516
Hydrotherapy, in epilepsy, 602-607
Hypnosis, catalepsy, 83-95; standardized suggestions for, 115 f., 442-444; suggestibility in, 279-286

Illusion, of movement, *468*, 647-649
Images, eidetic, 399-404
Imitation, in song birds, *464*
Individual differences, in consonance, 563 ff.; in limen for weights, 646 f.; judgments, 650; visual acuity, 63-71
Infant behavior, *492 f.*
Inferiority feeling, *502 f.*
Inhibition, in learning, 650
Insight, definition, *661*
Instinct, and intelligence, *668 f.*; in song birds, *464*
Intelligence, adult, *461*; and age of parent, 652; development of, *666*; factor theory, 645 f.; and instability, 653; and instinct, *668 f.*; theories of, *334 f.*, *338-341*, 608-620
Intensity, size and, 420-422; of tones, 528-542
Introversion-Extraversion, *see* Extraversion-Introversion

Journals, *Kwartalnik Psychologiczny*, 321; *Social Psychology*, 321

Kinaesthesis, in discrimination, 13
Knee-jerk, recording of, 295
Kohs test, scoring of, 450-452

Learning, adult, 429 f.; auditory, *vs.* visual, 370-388; in chicks, 651; cortical functions in, 652; inhibition in, 650; repetition and, 457-459; rational, *467*; spaced *vs.* unspaced, 591-601; theories of, *329*, *490 f.*; vocimotor *vs.* manumotor, 382 f.; whole *vs.* part, 591-601
Limen, absolute and differential, 389-398; area and visual intensity, 420-422; auditory, 1-16, 389-398; constancy of, 317-318; heat, 75, hue, 651; individual differences in, 646 f.; in optic disk, 215-220; terminal, 1-16; tonal attributes, 528-531; two-point, and localization, 446-449
Lissajou figures, 647-649

Localization, auditory, 14, 519-524, 650; and the two-point limen, 446-449; visual, 555 f.

Maze, *see* Apparatus; learning, factors in, 315-317; reliability of method, 439-442
Meaning, Gestalt theory, *660*
Meetings, Experimentalists, 469; Midwestern Psychological Association, 650-653; Mental Hygiene Congress, 147; Southern Society, 459-469
Melody, in memorizing, 184, 191
Memory, meaning and, 650; mode of presentation, 370-388; retroactive inhibition, 650
Mental hygiene, *169*
Mental set, in visual apprehension, 96-100
Method, constant stimuli, 402-404; order of merit, 652; questionnaire, *499 f.*; scale of values, 17-27
Mind, theories, *470-472*, *485 f.*
Motivation, *327-331*
Movement, apparent, *161 f.*, 222-223; in blind-spot, 222-223; illusory, *468*, 647-649
Muscle, contraction, recording, 584 ff.; tension and thickening, 295-297
Music, ability in, *475*; psychology of, 127-134, 561-572; revised notation, *490*; tests of ability, *475-477*

Nervous system, *342 f.*

Objective psychology, criticism, *343 f.*
Observation, attitude of, 345-369
Observer, in psychology, 320
Odors, affective value of, 17-27
Optic-disk, *see* Blind-spot
Organization, and context of, 175 f.; modes of, 177
Orientation, in animals, *168*; and asymmetry, 51-62; and dextrality, 54-62; in white mice, 413 f.

Pain, in burning heat, 77-81; thermal stimuli for, 423-429
Parotid secretion, dehydration and, 602-607
Pattern-vision, in rats, 652
Perception, of form, 63-71, 246-259; immediacy of, 621-627; in peripheral vision, 63-71; problems of, *483 f.*; of silhouettes, 431-435
Peripheral vision, acuity in, 63-71; form in, 246-259; physiology of, 258
Perseveration, of suggestion, 283
Personality, *503*
Personnel, college, *482 f.*
Phase, tonal attributes and, 519-543
Phenomenological description, 544-560
Philosophy, function of, *468*; history of, *166 f.*; and science, *462 f.*
Phi-phenomenon, in blind-spot, 222-223
Physiological, psychology, *504*; theory, 258
Posture, during sleep, 270-278
Practice, and auditory limens, 1-16; and fatigue, 628-630
Psychoanalysis, *504*
Psychogalvanic resistance, 652
Psychophysics, in differential psychology, 646 f.; order of merit method, 652

Racial differences, learning, *464 f.*
Reaction time, and extraversion-introversion, 412; relations of, *468*
Reading, training adults in, *429 f.*
Redintegration, in maze learning, 652
Reflex theory of thinking, *170*
Register, psychological, *153-156*
Relational reactions, 651
Relaxation, progressive, *473-475*
Religion, and ethics, *162-164*; and science, *466*; in universities, *169 f.*
Repetition, and omissions, 457-459
Reproduction, of associations, 177 f.
Rest cures, *473*
Retinal rivalry, 260-269
Retroactive inhibition, 455-457
Rhythm, as Gestalt, *164 f.*

Science, nature of, *149 f.*; philosophy of, *148-153*; and philosophy, *462 f.*; and religion, 466
Sensation, Mayer's 'residual,' 38-50
Sensationalism, and affection, 28-35
Sensory experience, immediacy of, 621-627
Sex differences, in association, 415-419; errors in determining, 140-147; visual acuity, 71
Shift, of mental set, 387
Size, and limen for intensity, 420-422
Sleep, diurnal *vs.* nocturnal, 277; motion pictures and, 651; posture during, 270-278; 651; temporal judgment during, 408-411
Social, facilitation, 320; experiments, *463*; psychology, *497*; sciences, *497 f.*
Space, fourth dimension, *488 f.*
Span, visual apprehension, 96-100
Speech, and hearing, *326 f.*; lip-reading, 650

Statistical methods, *336*, *338-341*, 645; attenuation, 235-245, 405-407; correlation, 609-614; exercises in, *502*; history, *498 f.*; misuse of, 142 f.; reliability of differences, *464 f.*; tetrad difference, 609-620
Structuralism, *vs.* Gestalt psychology, 134-140
Suggestibility, criteria of, 280 f.; trance *vs.* waking, 279-286; training in, 283 f.
Symbolic contexts, 198-200
Systematic psychology, *148-153*

Temporal judgment, individual differences, 650
Temporal patterns, rats and, 651
Tests, freshmen, *462*, *463 f.*; Kohs, 450-452; Stanford achievement, *504*; time-limit *vs.* work-limit, 101-104; will-temperament, *501*
Textbooks, of psychology, *479 f.*; *484 f.*, *486 f.*, *489*, *495 f.*, *496 f.*
Thirst, sensation, 606; true and false, 606 f.
Thought, and habit, *667 f.*; reflex theory *170*
Time, during sleep, 408-411; and space, *338*
Tones, attributes, 519-543; criteria of, 8 f.; volume of, 544-560
Tonus, measurement of, 581-590
Transfer, in learning, 202 f., 386; theories, *487 f.*

Vibrato, psychophysics of, 651
Visual acuity, anomalies, 287-294; individual differences, 63-71; intensity and, 287-294; in the periphery, 63-71
Visual, defects and acuity, 63-71; depth, 544-560; phenomena, *662 f.*; space, *662 f.*
Vocational psychology, *500 f.*
Volume, tonal, 14, 528-542, 544-560

Weber, ratio, for sounds, 393-398; sensory circles, 446-449
Will, free, *500*
Will-temperament, test, *501*
Work, curves, 628-630; decrement, *460*; the hand at, *494*; mental, and relaxation, *474*